The Journal of
NERVOUS and MENTAL DISEASE

VOL. 133, NO. 5
November, 1961
SERIAL NO. 948

CLINICAL JUDGMENT AS A DISCIPLINED INQUIRY[1]

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In the beginning was the Clinician. And the fogs of mysticism lay heavy upon the land. And the people suffered greatly, and cried out to the Clinician, Woe is unto us! But tell us, wise one, what kind of woe is unto us, and how can we escape therefrom? And the Clinician opened his mouth and spake large words, and great was the obscurity thereof. But behold, in the generations of those that sought righteousness, a prophet did come, and Pearson was his name. And his disciples did turn upon the seed of the Clinician, and smote them grievously, yea, even unto the loss of their grants. Now it came to pass that a judge arose in the land, and Meehl was his name. The Clinicians and Statisticians did congregate before him, saying, Tell us, oh master, which among us hath righteousness. Blessed are the peacemakers, said the judge Meehl, and he uttered his judgment. Whereupon the battle waxed more furious than ever, and the Statistician did seek to cut off the Clinician's third ear with his mighty formula. And the Clinician did seek to laugh his adversary to scorn, but his laughter was troubled, and hollow the sound thereof. Verily, he that held forth alone in the beginning was hard pressed at the end, for the Statistician respected not the Clinician's gray hairs.

The clinician did, of course, come first. Long before statistical and actuarial method was invented, people tried to figure things out as best they could, and those who were good at predicting future events were hailed as prophets. Fearing the unknown, man has always given great respect to those who claimed to know the future, and has held them in awe. It is understandable, therefore, that an aura of mysticism attached to the intuitive pronouncements of the clinician, who often honestly did not know how he knew what he knew, and who attributed his successes to all kinds of irrelevancies.

Later came the statistician, bright with arrogance, and haughty in the knowledge of his powers. Where the clinician relied on his fingertips and the seat of his pants to assess intangibles like intelligence, the statistician set about building objective tests to measure them. Steadily the frontiers of the statistician have advanced, often at the expense of the clinician's domain. Mathematics has drained the swamps and burned off the fogs of mystery in many indisputable cases. Small wonder, then, that the minions of the actuary and the statistician are becoming a little swollen with pride and ambition, and threaten the clinician with utter rout.

I trust I have used enough metaphors to convince you that I am, in fact, a real live clinician, and probably a soft and fuzzy thinker. Nevertheless, I want to make it clear, before going any further, that I applaud the efforts of people like Meehl (19)

[1] An abridged version of this paper was presented at a symposium on clinical judgment at the Twenty-fifth Psychological Research Conference of the San Francisco Bay Area, Palo Alto Veterans Administration Hospital, April, 1961.

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to clear away the haze, and look forward to the day when the mysteries of clinical judgment may be wholly laid bare. My sympathies are not with those of my colleagues who glory in their own inability to analyze or understand the processes of inference and judgment, even though I believe that, when the analysis is finished, many differences in kind will be found between the cognitive processes of the clinician and the procedures of the statistician. It can only help the clinician to have the processes of clinical judgment and inference made as clear and explicit as possible.

Further, it is good to focus close attention on the process of clinical judgment, because the issues involved are significant far beyond the purview of psychology—they get us into methodologic questions hotly debated in many of the behavioral sciences and, indeed, in the humanities.

Let me begin by restricting the meaning of the term. In contrast to the view of Hoffman (8), I would exclude from clinical judgment the very operations he studies: the combination of quantitative data. If the issues are defined as they are framed in Hoffman's paper, it is a foregone conclusion that the clinician is going to be left to choke on the dust in the wake of the formula. I have too much respect for the formidable powers of modern mathematics and high-speed computers to think that the cleverest and most experienced clinician is going to be able to excel at predictions based solely on quantitative data. In practice, however—that is, except in the special context of experimental studies—clinicians do not make any substantial use of such predictions. True, a good deal of the literature on the analysis of the Rorschach, the MMPI, and the scatter of the Wechsler subtests would give one such an impression; clinical judgment might seem to be a matter of mulling over numbers representing scores on these tests and various derivatives of them (ratios, differences, and what not). One sees statements that a larger number of M than average may indicate creativ imagination or delusional thinking, depen ing upon the configuration of other score In practice, however, the relevant con figuration is almost invariably one of *mean ings,* not quantitative scores—in this ex ample, qualitative aspects of responses tha indicate the presence of a thinking disorde or even a context of presenting symptom or aspects of clinical history. Again, I would not deny that I have seen impressive virtuosity on the part of people who de scribe a personality after just seeing an MMPI profile, a Wechsler scattergram or a Rorschach psychogram; but these are largely stunts, rarely systematically validated, and not a major device of clinical reasoning in everyday practice. But a computer could doubtless be programed to digest such configurations of numbers and their behavioral correlates, thereafter performing even greater prodigies of pattern analysis than any expert.

Instead, I believe that the essence of clinical work lies in dealing with *verbal meanings*—discursive symbols, if you prefer Suzanne K. Langer's term. This is the essence of free associations, dreams, autobiographies, social histories, TAT stories and other projective test data. To a lesser extent, clinicians work with presentational symbols such as figure drawings and expressive behavior, too. These kinds of material cannot be used at all without the interventions of a trained intelligence. For this very reason, those who wish to demonstrate the superiority of actuarial and statistical predictions try to define things so that both parties to the competition deal only with numbers. Indeed, unless some such proviso is made, it is impossible to set up a direct comparison of two different methods of predictively analyzing the *same* body of data. So much the worse for the comparisons; I agree with Lee Cronbach that little of scientific value emerges from such horse races.

As soon as we leave behind objective

t sts, demographic variables like age and l ngth of hospitalization, and miniature s tuations yielding quantitative measures of p erformance, we are getting into admittedly a arshy ground. It is fair to say that no o her field has solved all the problems of a ethod involved in the interpretation of s mbols. Suzanne K. Langer calls it "a f ld which usually harbors a slough of de- s ond for the philosopher, who ventures i to it because he is too honest to ignore i , though really he knows no path around i s pitfalls. It is the field of 'intuition,' 'deeper meaning,' 'artistic truth,' 'insight,' and so forth. A dangerous-looking sector, indeed, for the advance of a rational spirit!" (13, p. 92). Titchener stood with the early behaviorists in getting rid of the messy problem of meaning by excluding it from the province of scientific psychology. Meaning has made a modest comeback among modern behaviorists since the development of such techniques as Osgood's semantic differential, which get us to the comfortable realm of numbers again. By taking note of the fact that speech is a form of behavior, the superobjectivists have been able to include language and even thought in their theories of behavior. Note, however, that such psychologists stay as far as possible from interpreting verbal texts themselves; that is left to the subject, his response being taken as the operational definition of meaning.

I am not so negative toward such research as the tone of these remarks may imply; on the contrary, I use these methods myself and am all for them. It is clear, however, that the clinician uses meanings in quite a different way. When he makes a clinical prediction (and any assessment of personality or psychotherapeutic intervention is at least implicitly a prediction), he looks directly at the verbal texts[3] themselves and *interprets* them. In doing so, he follows in a long tradition of human scholarship, a tradition which in fact antedates science. Historians, anthropologists, sociologists, political scientists and also biblical scholars, philosophers, literary critics and other scholars of the humanistic disciplines are the clinician's brothers-in-arms in relying every day on the interpretation of meanings from verbal texts. On the hunch that there might be something to learn about the interpretive aspect of clinical judgment by forays into some of these other disciplines, I began work on this paper by reading some literary criticism,[4] history and anthropology, noting the methods the authors were using on verbal texts. What follows grows out of these observations, out of some methodologic reading, and out of my own clinical work, particularly interpreting the TAT and trying to teach such interpretation.

METHOD OF ANALYZING VERBAL MEANINGS

In dealing with verbal texts, the disciplined worker (in the sciences and the humanities alike) can perform a number of operations on the particular message itself, which we may call internal analysis, and others by reference to other materials—external analysis. Since I have deliberately left vague the boundaries that define a "message," the distinction between internal

[3] Most of the time, clinicians *listen* to what the patient says, in an interactive context of participant observation. It may not appear obvious, therefore (though I believe it to be true), that the mental operations performed by the listening psychoanalyst or psychiatrist are substantially identical with those of the diagnostic tester, reading and reflecting on his protocols. In this paper, I have deliberately disregarded the analysis of non-verbal data, which may play a large role in psychotherapy.

[4] Specifically, I undertook to discern the analytic operations performed by Henry Nash Smith (25) in his brilliant introduction to Mark Twain's *Huckleberry Finn*, and relied further on a similar reading of Auerbach (1), as well as on psychologic and other sources from the behavioral sciences. I owe a particular debt to Lerner (16) (especially the contribution by Erik Erikson). It is less easy to specify the many ways in which my thinking about these matters has been influenced by two years of work in content analysis, under the tutelage of D. P. Cartwright and A. A. Campbell.

and external is pretty arbitrary, and I attach little significance to it.

INTERNAL ANALYSIS

At least six kinds of internal analysis may be distinguished:

1) The content of meanings may be *summarized*. The analyst gets rid of the redundancy by a process of abstraction and selection. Eliminating redundancy implies the ability to perceive identities or similarities of meaning despite apparent differences; eliminating irrelevancy or the unimportant implies the ability to conceptualize—to recognize a meaning as an instance of a generalization—and it brings into play *standards* of importance which must derive from some theory. Summaries thus use two complementary aspects of conceptual thinking: the formation of generalizations or abstractions from a realm of particulars, and the particularization of an abstraction.

Like all the other analytic procedures to be described, they require the use of some theory, explicit or implicit.

2) One may *collate* a verbal message, examining it for internal consistency and inconsistency. The mental operations involved here are essentially the same as the ones just described. Such inconsistencies may be important cues to the historian or literary critic in his efforts to establish the authenticity of a document; to the therapist, they may be the very points at which to direct his interpretive efforts.

3) The analyst of verbal messages *interprets* or translates their content. In that case where he wishes to substitute sentences in one language for sentences in another, he follows a set of rules, most notably a dictionary, to tell him how to transform or replace individual words, and a grammar to tell him how to put the words together to make acceptable sentences. When he interprets a dream, some of the steps the psychotherapist goes through are logically identical with those of the linguist: he, too, has sets of rules that enable him to substitute a superficially very different text for the one originally presented. As Wittgenstein once said, "The internal similarity between things which at first might seem to be entirely different . . ."—for example, a sheet of music and the sounded notes—lies "in the fact that there is a general rule by which the musician is able to read the symphony out of the score" (13, p. 79). Thus, the task of interpretation requires the development of such rules for decoding encoded information. This type of analysis implies, of course, that human communications often or typically have more than one layer of meaning. A verbal discourse may transmit one set of ideas in its manifest content encoded according to one set of rules, but there may simultaneously be encoded, by a different set of rules, another (latent) which may be recovered by the application of these rules.

What is the relation between interpretive translation and conceptualizing? Both would be examples of Sarbin's (23) instantiation of modules (*i.e.*, classifying something as an instance of a general category). In conceptualization, we subsume something under a general conceptual category, generalizing a particular; in translation, however, we substitute an instance in one frame of reference for an instance in another, without subsumption or generalization. The simplest case, translating a message from one language to another, involves such fixed procedures that it is today possible to program a computer to carry them out with moderate success. But in the kind of symbolic translation that the clinician does, in analyzing dreams or projective test data, the rules are less fixed and must be applied more flexibly. A cigar may have oral or even anal significance as well as phallic; and sometimes it is only a cigar. How, then, does one tell which meaning to substitute, if indeed one should translate at all? The rules help by specifying occasionally in terms of context: thus,

i the cigar is mentioned along with an e mphasis on its color, dirtiness and smelli ess, and if it is one of several examples o similarly dirty and smelly things that c not share its oral and phallic implicat ons, then the anal interpretation is to be a cepted as the most likely hypothesis. The u timate criterion to decide whether to t anslate or not and, if so, how, is internal c nsistency. If we make the substitution, d es the interpretation as a whole make s nse, or not? That is, is it self-contradict ry, does it fit into a larger context? "Fitt ng in" goes beyond just the ascertaining of l gical contradiction; it involves also judgm ents of congruence and the formal eleg ance of the total structure that results. (See discussion of evaluation, below.)

At this point, many clinicians may object that there is a considerable difference between translating a single symbol and interpreting a whole style of life. In the former instance, the model of translation seems a good deal more applicable. It is true, I think, that the larger the body of verbal material being interpreted, the more judgmental the process, and the less obvious it is that translation is involved at all. Yet the psychoanalyst, reflecting back over many hours of the patient's production, and noting the emergence of a general pattern, let us say, of self-destructive rebellion, uses a number of individual translative steps in subsuming many superficially different episodes under the general category, which emerges in his mind as the one abstraction that expresses something true about all the incidents the patient has related. Each incident may have given rise to several possible interpretations, so that these clusters of possibilities affect one another by cancellation and reinforcement, the most recurrent ones then acting as a readiness later to perceive new episodes in terms of the emerging hypothesis. This, in turn, may be only one element in a constructed pattern; the finished "interpretation" involves other steps beside the one here called interpretation—notably, causal discerning and synthesis. (See below.)

4) The interpreter may analyze not the content but the formal and *structural* properties of the text. This may be done in a number of ways in different disciplines. The literary critic, for example, may apply a conceptual apparatus containing rhetorical and stylistic categories: the text is prose, a myth, it is anaphoric, paratactic, and so on. The anthropologist, interested less in style than in patterns of content, may find in a set of myths a recurrent pattern of binary opposition involving a balance of paired elements, each of which is dichotomously subdivided (15, 17). The clinical psychologist may note the brevity of a TAT story, the simple sentence structure, the stereotyped repetition of phrases, and the presence of circumstantial description, leading him to the inference of depression (22).

Note that the last two types of analysis involve what the content analyst calls *coding*: identifying an aspect of a communication (of its form or of its content) with a concept or category in an analytic outline, conceptual system or code. The discussion so far has carried the implication that, in either case, this is a procedure of typing or categorizing. But a second type of operation is often involved in either case: quantitative *rating* or scaling. The raw data are examined for their congruence with a concept, and a number is assigned representing the frequency with which a kind of instance occurs, the degree to which the fit is exact, or the intensity of its expression. Thus, for example, one may look for anger, and count all its manifestations in a text, or rate the degree to which a given statement approximates an ideal statement of "pure anger," or (conceiving of anger as a variate ranging from mild irritation to blind fury) rate the intensity of anger in a given bitter statement. According to Lazarsfeld and Barton, "There is a direct line of logical continuity from qualitative classification to the most rigorous forms of measurement, by

way of intermediate devices of systematic ratings, ranking scales, multidimensional classifications, typologies, and simple quantitative indices . . . One way to develop social science measurement is to *systematize* these commonly performed research procedures, by codifying exactly what successful researchers [read here, clinicians] do in carrying out these simpler forms of measurement and exploring their logical implications" (14, p. 155).

5) One may analyze a verbal message by reacting to it, and observing one's own reaction. This method, described by Murray (21) as *recipathy*, is a vital part of the clinician's kit of tools, especially in therapy. If the psychotherapist finds himself beginning to get angry at the client, he may well infer that the message being transmitted includes provocation. As Erikson has said, "The evidence is not all in if he does not succeed in using his own emotional responses during a clinical encounter as an evidential source and as a guide in intervention, instead of putting them aside with a spurious claim to unassailable objectivity" (5, p. 84).

6) An important procedure in analyses of verbal materials is the *discerning of causal relations*. There has been a good deal of discussion and controversy over the legitimacy of causal analysis in history, for example (26), and the traditional view of linear causality has been widely rejected on philosophic grounds, being replaced by a view of events as part of a nexus of functional relations from which no single cause or effect can be extracted. Nevertheless, such sophisticated models may be built up out of old-fashioned observations of single causal relations, and these in turn are *not* exclusively Humean inferences on the basis of statistical contingency.

Following the Gestalt psychologists (*cf.* 12), Michotte (20) and Heider (6) have made experimental demonstrations and analyses of the perception of cause: people do react to single events with a certain kind of structure by *perceiving* that A causes B. In such an instance, A and B may have not only a certain temporal sequence, but may also share common meanings, move in specifiable ways, or involve elements that stand to each other in the relation of an intelligible transformation (*e.g.*, homologous shapes, but B′ is smaller than B: the observer perceives that the intervening event A causes B to shrink). This kind of discerning of causal relations plays a critical role in all science in the setting-up of hypotheses and the interpretation of findings. (See below.) There has been no such experimental work that I know of in the realm of verbal materials, but I believe that the cognitive processes involved are basically the same as in the realm of non-verbal events. To some extent, there must be added another cognitive operation: from the verbal text, the analyst reconstructs images of events in which causal relations occur. (For another type of causal analysis, see below, discussion of *comparison*.)

EXTERNAL ANALYSIS

An approximately equal number of techniques of external analysis may be listed also:

First, the text under consideration has been *selected* from some larger population of possible data; the technique of selection is an easily overlooked contributor to the ultimate interpretation, and a mode by which clinical judgment is exercised. If the message in question is the statements of a patient in a therapeutic hour, various external factors bring about the selection, which is thus not obvious. If it is a historical document chosen for study among myriad such materials, it is plain that the historian has selected and that his principles of selection will vitally affect his conclusions. One has to ask: Is the sample of verbalization a sufficient basis for the further analysis proposed? Did the method of selection insure representativeness of the population sampled? In history, the selec-

t on is made on more or less explicit theo-
r tical grounds. Moreover, "the documen-
t ry method of historians is reflected in the
c ioting or citing of statements to illustrate
t ie pros & cons of moot points" (26, p.
1 30). "Citations are selected in terms of
t eir relevance to the thesis," not in accord-
a ice with some sampling procedure. Thus
S llars (24) cites and analyzes toasts, per-
s nal correspondence and schoolboys' essays
f r evidence in support of his thesis that
s avery was a conflict-ridden moral problem
f r the South; but the historian never pro-
c eds by *systematically* sampling all such
n aterial and then extracting all themes
r elevant to slavery. Instead, the historian
n akes a case, trying to avoid the self-de-
ception that results from the systematic
ignoring of contrary evidence. In anthro-
pology and sociology, the method var-
ies: at the most sophisticated extreme
there are content-analysts like Lazars-
feld, who take considerable pains to
take a probability sample when ana-
lyzing, say, the newspapers of a coun-
try to extract its basic values. At the
other extreme are workers who are unaware
of the problem, and sample simply accord-
ing to convenience and availability: thus,
the one Indian who can speak English may
become an unwary anthropologist's inform-
ant, and provide a seriously biased sample
of information about customs. In clinical
psychology, diagnostic assessment usually
develops its own data, and the diagnostician
uses them all, overlooking the fact that he
too is sampling. Has he given enough tests?
Has he given the appropriate ones? Has he
supplemented them with the most advan-
tageously chosen interview questions? Here,
however, the principle of random sampling
has but narrow relevance; selection (like
summarizing, which it closely resembles)
must be guided primarily by relevance to a
theory.

A second form of external analysis con-
ists of reconstructing the chain of circum-
tances that brought about the verbal mes-
sage itself, or the real events it refers to (if
any). This *genetic* analysis obviously plays
a large role in history, in literature, in politi-
cal science and in many other disciplines
beside clinical psychology. Central to it is
the method of discerning causal relations.
(See above.)

A third—and most important—type of
external analysis is *comparison* with other,
similar texts, bringing out similarities and
differences of content or structure, especially
in conjunction with other known facts about
the verbal samples compared (*cf.* collation).
This is obviously J. S. Mill's joint method
of similarity and difference. Tomkins (27)
was the first to demonstrate the applicabil-
ity of Mill's canons to the interpretation of
the TAT. (This might just as well have been
called a method of internal analysis, but
one story may be taken as context for an-
other, as well as one *S*'s TAT as context for
another *S*'s whole TAT.) William E. Hen-
ry's (7) concept of "negative content" is
relevant here: one may use knowledge of
what usually occurs in a series of simi-
lar communications to help one notice
what is significantly *not* present in the text
under analysis. Such comparisons may aid
almost any of the kinds of internal analy-
sis mentioned; the central principle here is
that of statistical inference. This method is
obviously central to literary criticism and
related fields, such as hermeneutics and
the anthropologic analysis of myth. It is
also closely related to two other methods of
external analysis I want to mention.

The fourth such method is to examine a
verbal message in relation to *other types of
data* from the same source, looking for com-
mon elements. Thus, in a paper referred to
above (24), the historian sought common
elements in different types of documentary
materials as well as (implicitly) consider-
ing, for example, an after-dinner toast in re-
lation to what is known about the genre. To
take an example from psychology, one ap-
proaches the analysis of a projective test
from the standpoint of other kinds of in-

formation about the patient, such as the affect and other non-verbal behavior accompanying the responses, demographic characteristics, presenting complaints, objective test profile; the clinician seeks generalizations that will cut across or at least not be inconsistent with all types of information.

Fifth, a message may be considered in relation to a set of external (usually *evaluative*) standards. If it is literature, the critic may, in elucidating various aspects of its structure, adjudge them more or less good in an esthetic sense; if it is a letter written by a historical figure, the historian may cite it as an example of the man's militarily relevant bravery; if it is a TAT story, the psychologist may rate its genuineness or creativity. All of these are basically value judgments, and they may play a role in the work of scientific as well as humanistic disciplines. A non-evaluative instance of this type of analysis is clinical diagnosis, in which the results of preliminary (or primary) inferences from the raw data are compared with a set of ideal types: the standard diagnostic syndromes. The person who produced the verbal messages being analyzed may then be categorized ("pigeonholed"), or he may be characterized by the degree to which he approximates various diagnoses and the particular characteristics he shares with them.

According to the methodologists of the *Geisteswissenschaften*, a critical point of difference between natural and "cultural" or social sciences was the central role of the concept of value in the latter (cf. 9). Even so clear a thinker as Max Weber considered this to be a basic differentiation. How, then, can it be said that value judgments play a role in natural science? First, it is obvious that much of the subject matter of social sciences is made up of values, unlike the natural sciences; anthropologists and sociologists are much concerned with the evaluative premises and systems (ideologies, religions and so on) of various groups of people. Yet as long as a value is treated as a fact worthy of study, like a myth, drea or superstition, it can be studied objective and no methodologic difference emerg Second, value judgments are used eve day in the naural sciences in much the wa described above. The scientist is constant evaluating his work in terms of pragmat values: Does an experimental approach pa off? Does an instrument work? And h evaluates his theory against sets of stan ards like simplicity and elegance. Wh n some physicists recently overthrew parit evaluative considerations played a substantial role: the assumption of parity, that nature had no preference for right- or left-handed rotation, grew out of essentially esthetic or formalistic reasoning. In mathematics and theoretical physics, the facts are considered to make most sense, the elements of a theory fit together best, if organized in an esthetically pleasing fashion. Thus, esthetic criteria of evaluation are important in these highly advanced scientific disciplines, where they are usually referred to as elegance. Simplicity (parsimony), balance, and clarity of organization are component values of elegance. Finally, theory is used in all sciences in an essentially evaluative way, in decisions about scientific *importance*. The decision that a phenomenon is worthy of investigation because of its bearing on a theoretical issue is an easily overlooked value judgment.

The final step in either the internal or the external analysis of clinical data—both discursive and presentational symbols—is *synthesis*: bringing the pieces together into a coherent verbal formulation capable of being grasped and used by another human being, in the sense that a therapeutic interpretation, or a case formulation in a few prose paragraphs, is more understandable and memorable than a Q-sort. This function requires clinical judgment of the broadest grasp, guided by logical considerations of internal consistency, to a very minor degree by empirical rules, and largely by theory. For an excellent example, which respect

the complexity of real clinical situations, see Erik Erikson's account (5) of how he interpreted a patient's dream. In it, he demonstrates how the clinician can examine a verbal message simultaneously from many points of view and in a number of relevant contexts, including an empathic assessment of the patient's affective condition, and can pull all the pieces together in a strategic verbal intervention requiring clinical judgment of the highest order. This synthetic phase involves a good deal of what I have called *secondary inference*: the drawing of conclusions on the basis of primary inferences from the data (*cf.* Colby (3) on "1st and 2nd order inference"). Though I have been speaking only in terms of clinical psychology (and related disciplines like psychoanalysis) in discussing this final, synthetic step, it obviously enters into the analysis of verbal materials in the other scientific and humanistic disciplines as well.

SIMILARITIES OF PROCEDURES IN ALL DISCIPLINED INQUIRY

So far, I have tried to draw occasional parallels to procedure in other behavioral sciences and humanities that use the interpretation of verbal information. Does the fact that meanings are discerned put the cognitive operations of these workers on a different methodologic plane from those of scientists who are not dealing with verbal materials? The German philosophers of half-a-century ago who made an ill-starred attempt to develop a special and separate methodology for the cultural sciences thought that the use of meanings was a fundamental point of difference (9). It is not, because the grasp of linguistic meaning (including values) is a special form of perception, and all science involves perceptions of human observers, followed by cognitive manipulation of symbols.

The basic observational operation in physics is said to be pointer-reading. A machine performs a measurement, but the experimenter has to receive the information generated and process it. Of course, there are now automatic machines for processing data, even in psychology: you can hook *S* into a polygraphic set-up, and the electrical impulses will be automatically measured, summed, squared, correlated and factor analyzed. Yet ultimately, *E* has to enter, to receive and decode or interpret the information—no matter how sophisticated the machine that generates it, no matter how far removed it is from the elementary operation of laying a meter stick on something; what *E* does is essentially the same. He must recognize the congruence of two discriminable entities (*cf.* recognition of verbal meanings). In the simple situation of measuring length, he recognizes the identity of the end of what is to be measured with a corresponding point on the meter stick (the other ends having been aligned—also an operation involving discrimination and recognition); in reading a pointer, he must discriminate and recognize the location of the pointer at a numerical position on the scale. If the measuring machine is much more complicated and automated, he still has to be able to discriminate and recognize the output, whatever it is.

Discrimination is a selective process, really quite complicated. *E* has to know where to look; he has to direct his attention there, not being distracted by the surround; he has to be physiologically capable of distinguishing figure from ground (pointer and numeral from dial-face; end of object from the physical context in which it is embedded). Ordinarily, in full illumination and with the kinds of examples that come most readily to mind, such discrimination is so smoothly and effortlessly performed that we overlook it or think it radically different from discriminating verbal meanings. Yet suppose you want to measure the length of a cloud or some other object without obviously demarcated boundaries; the operation of discriminating the end of the object from its surround will be extremely difficult, and the measurement

approximate and inexact (unreliable). If you seek to measure a nematode, like a dead vinegar eel, against a ruled glass scale in a microscope, the lack of contrast may make the discriminative operations frustratingly difficult to an untrained microscopist. The point is that even the elementary operation of distinguishing the basic items of information from their contexts may differ considerably from one situation or example in natural science to another, and not infrequently is as tricky and judgmental a business as the discrimination of verbal meanings. Both involve separating information from noise.

Logically, however, it is conceivable that *E* discriminates the necessary discriminanda, but does not recognize them. Surely it happens a great deal in scientific work that *E* can and does see differences between phenomena but ignores them or considers them outside the realm of his interest because they do not correspond to anything of importance (*i.e.*, relevance to his scientific aims). Recognition is the operation of establishing correspondence between something perceived and something conceived, between discriminated sense-data and a concept stored in *E*'s memory. In certain kinds of aphasia, pathology nicely separates these two phases: the patient can discriminate, let us say, a chalk mark on a blackboard, but has no idea of what it means; he cannot recognize it.

Failures of recognition can occur in several ways. There can of course be a failure of the presupposed *sine qua non*, discrimination. Discrimination can occur, but *E* may not have the requisite concept in his memory: he is ignorant of a distinction, untrained in recognizing something of importance. Both of these requisites may be present, yet something may block the scanning process by which the memory is made available for checking against the perceptual process for degree of fit. (In the above example, organic lesions may interfere; fatigue, drug-effect, illness, emotional upset and other adventitious sources of mental inefficiency or noise may also prevent this process; the discrimination may be too fragmentary or incomplete to make the process fully possible; *E* may be too rigid to perform the scanning operation, or have some other such defect of personality as a cognitive style (11) of demanding too exact a degree of correspondence, or of accepting even gross partial identity as a satisfactory fit—sharpening vs. leveling, or something like it; the memory traces of the relevant concepts may be insufficiently well differentiated or too poorly established: *E* may not have a good grasp of what he is looking for, may not fully understand the concept.) In some situations, the discrimination may be difficult and the concept not very precisely definable, *e.g.*, in reading X-rays: can precise criteria for the recognition of an ulcer crater be set down, since every duodenum is shaped differently from every other, no two ulcers occur in precisely the same spot, and no two are visualized in precisely the same manner? It takes training, with a lot of ostensive definition supplementing the basic conceptual one, before the young radiologist develops fully usable capacities to discriminate the relevant shadows *and* to recognize a crater when he sees one.

The hypothesis I am proposing is that many of the operations performed by analysts, clinical psychologists, anthropologists, linguists, sociologists and scholars in a number of other behavioral sciences *and* disciplined humanities are exactly analogous or even homologous to discrimination and recognition as described above. Obviously, all such disciplines involve the selection of data (discrimination), and focusing on particular objects of study; they also involve conceptualization: the application of concepts having operational definitions to the discriminated data (recognition). The other operations used—classification, abstraction, discerning of relationships (*e.g.*, cause or functional dependence; con-

t adiction or inconsistency; similarity or p artial identity; meaningful implication)—m ay be viewed as more sophisticated forms o these two. They certainly are used in the n atural sciences as they are in the be- h avioral ones.

Physicists and other experimenters in the "h ard" sciences have a great deal of re- s pect for what they call intuition. By this t rm they do not mean anything super- n atural like revelation; but they do not m ean unrecognized statistical inference, e ther. Intuition in this sense refers to the c reative process by which new theories and experimental ideas arise. If we look at it m ore closely, we will find that it involves many of the operations described above for dealing with verbal texts: discerning of causes not through statistical association, but by meaningful congruence; the manipulation of theoretical ideas by means of analogies and constructions to form new propositions; judgments of the probability of pay-off and of relevance of facts to hypotheses. The nuclear physicist Deutsch (4) writes, "The decisive intuition of the experimenter is really the ability to *recognize relevance* in the evidence presented by the experiment." This statement points to another phase of the work (besides that of hypothesis-formation) when judgment plays a large role: after the experimental data have been presented. In the discussion of results, *E*s discern causal relations, look for what makes sense, seek internal consistency, or consistency with a theory. In all sciences, there is this phase of reflection on the data obtained, which may even get pretty speculative; and I want to point to the methodologic identity of these cognitive operations with those of the clinician, literary critic or behavioral scientist in interpreting meaningful discourse.

The implicit goal of those who glorify statistical prediction seems to be to exclude intuition and judgment from science entirely, replacing it by cookbook rules, logical analyses, or mathematical formulae solved by electronic computers. But: "There is no recipe, no rule of procedure which will permit the unimaginative experimenter to consider all of these [*i.e.*, the necessary and the relevant] possibilities by applying systematic logic analysis to all aspects of his apparatus" (4). Again, this is an experimental nuclear physicist who is speaking about investigation in his own field. No—imagination, creativity and intuition will always remain essential qualifications for scientists of any kind, including the purest mathematicians. We had best, therefore, try to understand this phase of scientific work, instead of minimizing it as if it were something to be ashamed of.

THE ROLE OF PREDICTION

It is sometimes said (*e.g.*, **23**) that the hallmark of science is its use of prediction; to establish the utility of its propositions, clinical judgment (like the unscientific humanities) has only the inferior criterion of internal consistency at its disposal. Let us begin a consideration of relations between clinical judgment and prediction by examining these criteria.

Internal consistency is no stranger to science; it is used in all sciences at the stage of constructing theory. Much of what Einstein contributed to physics was of just this sort: he had the capacity to educe generalizations or to abstract in a way that brought out the internal consistency of phenomena and concepts long thought unrelated or even contradictory. On a homelier level, any intelligible thought whatever requires some attention to internal consistency, for the secondary process gives way to the primary when contradiction is tolerated, and logical conclusions cannot be reached.

When contrasted to internal consistency, predictive validity seems at first sight to be a very different kind of criterion of truth. But is it? One forms a hypothesis on the basis of one body of data (or on the basis of a theory, when there is at least

indirect reference to the data involved in its development), predicts that this state of affairs will continue to hold in a new sample of data, and tests the prediction. But *the test of predictive validity is nothing more than establishing the degree of internal consistency within the combined body of (1) the data (and theory) on which the prediction was based, and (2) the newly obtained data.* Logically, there is no difference between the two criteria; therefore, what looks like prediction can be converted into a search for internal consistency, and vice versa.

Thus, the clinician can and should accomplish something like a cross-validation by dividing the data within the sample available to him: predictions based on the Rorschach may be tested in the TAT, and it is even possible to form hypotheses on the basis of responses to one card or picture and to test them on the immediately following material within the same test. After every interpretation or other intervention, the patient's subsequent behavior enables the therapist to test his hypotheses. Moreover, routine clinical work offers opportunities to collect different kinds of data to verify predictions: hypotheses based on a diagnostic study may be checked in the course of therapy; finding signs of an unsuspected brain lesion, the testing psychologist may suggest a neurologic examination which in turn verifies his diagnosis.

While there is no essential difference between internal consistency and predictive validity, there are certain practical advantages to formal cross-validation or predictive study. The predictive experiment generally uses a sample of *Ss* different from the ones on whom the prediction was based, and involves gathering data at a different time and possibly in different ways. The true cross-validation is in this way superior to splitting the data gathered at one time and treating the halves as replicates of the experiment: subtle sampling biases that affect the latter are not so likely to affect cross-validation, and an other unknown parameters of the origin l result are less likely to be repeated (2 . The clinician can rarely go beyond wh t amounts to cross-validation, though son e of the data available to the clinician wh o has a battery of tests and some intervie w material are so different in kind that th e prediction from one such source of data t o another must be mediated by the sam e processes of constructing and operating a theoretical model that are involved in th e design of good original experiments. And, granted an adequate theory, the clinician may predict phenomena not previously observed: his investigation may thus not be merely a cross-validation.

Granted the need to supplement the disciplined analysis of a body of data by predictive verification, are there any differences between the processes of clinical and statistical predicting beyond the differential amounts of error they may contain? Yes—despite the arguments of those who claim that clinical predictions are only unsystematic and therefore faulty actuarial predictions (18, 23). I agree with Meehl (19) that the clinician generates many of his predictions (not all; some are actuarial) by means of a creative cognitive act. "Essentially the judge constructs a model of a personality and deduces the appropriate kind of behavior from that model. This means that he must take data of whatever kind he can get and put together a total picture of a personality that would have produced such data" (10, p. 210).

Like any other kind of creative thinking, the thought processes of the predicting clinician may involve regression in the service of the ego: in creating new ideas, new forms, new theories, we can use primitive, illogical, even magical kinds of thought-processes in flexible alternation with more rational ones. An N of 1 is obviously not a reliable base for a hypothesis, yet the hypothesis may prove true. Bu t the fact that the primary process ofte n

disregards logic and reality does not mean that it necessarily leads to error; it means only that hypotheses must be tested. No analysis of the process can settle the question of how "credible" clinical inferences are, no matter how dubious their major premises; only the outcome of the predictions can tell us about their truth.

By contrast, the method of statistical prediction is formed on the model of the secondary process—reproductive, not productive thought. Statistical methods can never create anything new, and thus can predict only recurrences of old patterns of behavior. (Since so much of behavior *is* repetitive, statistical prediction works quite nicely most of the time; that is not the point, in this argument.) I am not saying that *statisticians* cannot create anything new; on the contrary, many of them are highly creative. But as the statistician devises new theories, new techniques, new types of formulas, he does not use actuarial procedures. He does not limit himself to the approaches used successfully by Pearson, Fisher and other statistical innovators of the past, applying them in a mechanical way to his new problems; he knows perfectly well that he would get nowhere that way. Instead, he proceeds just as intuitively as the clinician or the physicist: he daydreams about his work, mulls his problems over, sleeps on them, works by fits and starts of inspiration. He just says nothing about that phase of his work in symposia on clinical judgment.

Though I freely concede the need for clinical judgments to be subjected to the test of prediction, I very much doubt that this is the way to refine or improve the process of judgment itself. Some clinical psychologists have perhaps been too much impressed by the principles of test-construction, too ready to generalize them to the status of guides for other ways of obtaining and dealing with data. The statistical study of reliability and validity (the latter through predictive studies) can be helpful in letting you find out where you are, but it offers precious little aid to your efforts to get further ahead. I have come to this view from a rather disillusioning experience. When Luborsky and I (10) were engaged in our final predictive study of methods for selecting psychiatric residents, we decided to make many specific predictions on the basis of interviews and projection tests and verify them, in the hope that we might learn more about how to improve clinical judgment. But when our criterion data were in and the predictions were tested, we found that the numbers told us very little: judgments from a single test, let us say, were slightly more valid in this area than that, but how we went astray or why we could at times make correct predictions remained obscure.

Almost 25 years ago, H. A. Murray (21) wrote that the clinical mind of the research worker was "psychology's forgotten instrument," one that could produce greatly if we could only learn how to use it. Perhaps so little progress has been made in refining this instrument because most of the effort to do so has mistakenly treated the clinical judge as if he were a test, and because psychologists have neglected to study the ways their fellow-scholars in other fields have attempted to discipline their analyses of verbal meanings: to carry them out in a systematic, orderly and reproducible fashion.

SUMMARY

Clinical judgment is the informal application of the creative, inquiring intelligence to human problems as principally conveyed through words. The methods it uses are in large part different from those of statistical prediction, although they are not by that token mysterious, unanalyzable or sacrosanct: they are the operations used in all sciences and in all kinds of disciplined human inquiry as hypotheses are formed or as the scholar reflects on the meaning of what he has found. The clinician should

make every effort to test his hypotheses by generating concrete predictions and verifying them on new samples of data. But he should also strive to understand, to discipline and to improve the analytic and synthetic processes of clinical judgment. In doing so, he can get more help from studying the methods of other disciplines that deal with verbal meanings than he can from predictive experiments themselves.

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THE PROCESS-REACTIVE DISTINCTION AND PROGNOSIS IN SCHIZOPHRENIA[1]

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Recently there has been an upsurge of interest in the alleged distinction between "process schizophrenia" and "reactive schizophrenia." Many writers, including Hellak (3) and Langfeldt (12, 13), have suggested such a distinction, although they have used varying terms for it.

The distinction is usually justified by the differing prognoses of two symptom patterns. Briefly, a process (or "typical") schizophrenic is said to be characterized by an inadequate pre-psychotic adjustment, with little interest in other people or the activities of life. The psychosis develops gradually from this pattern, with no identifiable precipitating stress. The symptoms usually include affective flattening with a clear sensorium. The prognosis is poor, and the disorder follows the deteriorating course described by Kraepelin for dementia praecox. The reactive (or "atypical") schizophrenic is said to be characterized by a fairly normal pre-psychotic adjustment, the psychotic symptoms appearing suddenly in response to severe stress. The symptoms include a clouded sensorium and marked affective display. The prognosis is good. It has often been suggested that process schizophrenia has an organic basis and reactive schizophrenia has a psychogenic basis.

Langfeldt reported marked success in predicting the outcome of psychosis on the basis of his differential diagnosis between these two types, as determined by clinical judgments. Wittman (29, 30) attempted to render the differential diagnosis more objective by offering the Elgin Prognostic Scale (hereinafter called EPS), consisting of a number of items which add up to yield a single score. Similarly, Phillips (23) developed the Prognostic Rating Scale, designed to predict outcome on the basis of characteristics of the "type of schizophrenic which will improve" and the "type which will not."

Several investigators have used the EPS to separate process from reactive schizophrenics in order to compare them on some other variable (2, 5, 7, 10, 19, 26, 32). In all of these studies the assumption was made that the EPS adequately distinguishes process and reactive patients. Yet the scale itself seems not to have been studied since Wittman's original reports.

The present report considers the value of the EPS, specifically in relation to its prognostic validity and the relationship of marital status both to EPS score and to prognosis. It also discusses the meaning of the process-reactive distinction. The concern with marital status was suggested by some recent observations of Garmezy *et al.* and the Phillips Scale.[5] They found that the classification by the Phillips Scale according to good or poor pre-morbid adjustment overlaps marital status to a large extent,

[1] This study was supported by Research Grant M-3481 from the National Institute of Mental Health, U. S. Public Health Service.

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with married men the "good pre-morbid" patients and single men the "poor pre-morbid" patients. Since both the Phillips Scale and the EPS were designed to predict outcome largely on the basis of pre-psychotic adjustment, the question arises as to whether marital status alone may be closely related to EPS scores, and whether it may predict the outcome of schizophrenia as well as EPS does.

The EPS, as originally advanced by Wittman (29), consisted of 30 items, 25 of which called for a rating on an item of pre-morbid adjustment, and five of which were concerned with presenting symptoms. The items were assigned various weights according to Wittman's clinical judgment. Later, on the basis of her experience in using the scale, Wittman reduced the scale to 15 items on social history and five on presenting symptoms. Although there were several rating points on each item, Wittman offered defining labels only for the end-points of the rating continuum of each item. Becker (1) remedied this deficiency by offering definitions for intermediate points of Wittman's items, and reduced the range of points on most of the items. Becker's revision of the EPS was used in the present study.

PROCEDURE

All subjects were obtained from Cook County Hospital Mental Health Clinic, a receiving hospital for psychiatric patients. (Patients are kept here only one or two weeks, after which they usually are either discharged or sent to one of seven State hospitals.)

The *Ss* were 106 males diagnosed as schizophrenic who showed no indications of mental deficiency or organic brain damage. Range of age as of last birthday was 17 to 54 years, with a mean of 29.7 years. Sixty-seven were white and 35 were Negro. There were 65 at first admission, and 31 at second or third. The latter 31 patients averaged five months of total prior hospitalization. Each *S* was rated on the Becker revision of the EPS, using the social history which had been written by the hospital social worker after an interview with one or more members of the patient's family. In most cases, the five items of the scale concerned with presenting the clinical picture were rated on the basis of a brief interview by the rater with the patient within a few days of admission; occasionally the psychiatric description of the patient had to be taken from the record instead. Most of the patients were receiving tranquilizing drugs at the time of the interview. When there was insufficient information to rate an item, pro-rating was used; if it was necessary to pro-rate more than three items, the patient was not included in the sample.

Somewhat later, an investigator made contact with each hospital to which the patients had been transferred to determine whether they had been discharged within nine months of admission. This discharge information is the criterion of prognostic power used in the present study. The information was successfully obtained for 105 of the original 106 patients. (The remaining patient died before nine months had passed.)

RESULTS

Wittman (29), following Langfeldt (12), viewed the process-reactive distinction as a strict dichotomy of types of schizophrenia. Some other writers (*e.g.*, 3, 8) view the terms merely as convenient names for the end-points of a continuous distribution. The statistical analyses of the present paper assume a continuum. However, the conclusions reached should be applicable to the assumption of a dichotomy.

The Relation of EPS to Discharge: Biserial correlations were computed between EPS scores and discharge status for each monthly interval from one to nine months after admission, by which time 50 per cent of the patients had been discharged. These values and the corresponding significance

TABLE 1

Biserial Correlations of EPS to Discharge Status at Monthly Intervals

Month	r_{bis}	p
1	0.14	0.26
2	0.09	0.48
3	0.10	0.39
4	0.14	0.26
5	0.16	0.18
6	0.29	0.02
7	0.26	0.03
8	0.34	0.01
9	0.38	0.001

levels are shown in Table 1. A significant relationship ($p < .05$) did not appear until the sixth month after admission. By the ninth month the correlation was .38 ($p < .001$). Although the relationship is clearly a significant one, it is low for purposes of prediction.

To determine which items[6] contributed toward the prognostic power of the scale, biserial correlation was computed between the score on each item and discharge status at the nine-month period. In the computation of these values, *Ss* for whom a given item could not be rated (and who thus had been pro-rated for the total score) were omitted. These values are shown in Table 2, together with the corresponding probability values. Only 11 of the 20 items were significant at the five per cent level.

Relation of Marital Status to EPS: The mean EPS scores for the three marital groups were: married, 33.0; divorced or separated, 49.4; single, 55.7. There was an overall significant difference between the three groups ($p < .001$), as shown by an analysis of variance. Comparisons of pairs of groups by *t*-tests (two-tailed) showed that the married patients differed from each of the other two groups ($p < .001$ in both cases), but that the single did not differ significantly from the divorced or separated (.10 $> p > .05$). Table 3 shows the relationship between marital status and EPS score when the latter is divided into approximate quarters. (The specific cutting points were chosen so as to place all tied scores in the

[6] Since the items in Wittman's (30) published version of the scale were not numbered, Becker's (1) numbers are used here. The reader may readily identify the item in Wittman's version by its defining label.

TABLE 2

Relationship of EPS Items to Discharge Status Nine Months After Admission

Item		N	r	p
A	Defects of interest vs. definite display of interest	105	0.26	0.04
B	Insidious vs. acute onset	104	0.26	0.03
C	Shut-in personality	92	0.09	0.50
D	Schizothymic vs. syntonic personality	105	0.15	0.21
E	Range of interests	105	0.25	0.04
F	Constitutional bias	99	0.28	0.03
G	Low energy tone	105	0.15	0.23
H	Asthenic build	92	0.31	0.02
I	Heterosexual contact	102	0.30	0.02
J	Marked academic interests vs. active interests in sports	91	0.15	0.27
K	Careless indifference vs. worrying, self-conscious type	105	0.28	0.02
L	Exclusive stubborn traits vs. insecurity and inferiority feelings	104	0.10	0.43
M	Toxicity of exhaustion	104	0.04	0.75
N	Precipitating conditions (situational reaction)	102	0.09	0.46
O	Duration of psychosis	102	0.32	0.01
P	Inadequate affect vs. emotional instability or appropriate affect	105	0.37	0.002
Q	Hebephrenic symptoms	105	0.24	0.05
R	Ideas of influence	105	0.12	0.33
S	Physical interpretation delusions	103	0.15	0.22
T	Atypical symptoms	105	0.26	0.03

TABLE 3

Relation of Marital Status to Score on EPS for 106 Schizophrenics

	EPS Quarter*			
	Highest ($N = 27$)	Next ($N = 27$)	Next ($N = 25$)	Lowest ($N = 27$)
Married	0	1	9	16
Divorced or Separated	5	8	6	4
Single	22	18	10	7

* The quarters are approximate: exact cutting points were chosen so as to avoid tied scores.

same groups.) As seen there, marital status and EPS appear to be highly related, but not perfectly so (chi-square = 37.20; df = 6; $p < .001$). This high relationship is especially interesting since the EPS, unlike the Phillips Scale, contains relatively few items directly dependent upon marital status.

Relation of Marital Status to Discharge: Nine months after admission, the discharge rates for the three groups were: single, 39 per cent; married, 73 per cent; divorced or separated, 52 per cent.

To compare marital status and EPS scores as to prognostic power, one might dichotomize the patients by both predictors and compare the percentages which are predicted accurately by the two dichotomies. In dichotomizing marital status, one finds what seem to be adequate theoretical grounds for lumping the divorced or separated patients with either of the other two groups. One might lump them with the married patients because they had at least once in their lives shown evidence of some sort of heterosexual adjustment. This apparently was the logic of Phillips (23) who, in constructing his prognostic scale, assigned relatively good pre-morbid scores to divorced and separated patients. On the other hand, one might lump the divorced or separated with the single patients, on the grounds that neither had succeeded in maintaining a marital adjustment.

If the scores on the EPS reflect a continuum rather than an underlying dichotomy, it is an arbitrary decision where one places the cutting point for predicting good vs. poor outcome. To compare meaningfully the prognostic validity of EPS with that of marital status, one might choose the cutting point so as to predict the same proportion of discharges as is predicted by marital status—otherwise, the chance distribution of prognostic accuracy would differ for the two predictors. For example, the present sample included 25 per cent currently married and 75 per cent not currently married. The prediction that the currently married would be discharged and the unmarried would not be discharged yielded 61 per cent accuracy at nine months after admission. For the comparable prediction—that the low 25 per cent on EPS would be discharged and the high 75 per cent would not be discharged—the accuracy of prediction was slightly less (56 per cent).

On the other hand, 47 per cent of the patients either were married or had been married at one time in their lives. A prediction that these 47 per cent would be discharged and the remaining 53 per cent would not was 62 per cent accurate at the nine-month interval. A prediction using a comparable cutting point on the EPS was slightly better (65 per cent accurate). Thus there does not appear to be much difference in the predictive value of marital status and total EPS score.

One might expect the prognostic validity of marital status to have been hindered in this study by the fact that there was no selection by age, so that the sample included some young men who were below the usual age of marriage. The median age of first marriage for American males is 22.6, according to *Vital Statistics of the United States* (28). Of the 105 patients in the present sample, 19 had not yet attained their twenty-third birthday; of these, two were married and 17 were single. The prognostic accuracy of marital status was computed again at the nine-month interval for the 86 patients who were 23 or older. The values were 62 per cent accurate prediction for current marital status, and 63 per cent for married once vs. single. In both cases, the prognostic accuracy was raised by only a single percentage point over the comparable values for the entire 105 patients.

DISCUSSION

The present results indicate that EPS predicts discharge slightly better than chance, but only about as well as marital statu

alone. One should bear in mind, however, that discharge status is probably not an ideal criterion of recovery. Many variables other than degree of improvement may influence whether a patient is discharged. Discharge status does have an advantage over criteria involving ratings of improvement in that it is a simple objective fact, and is not dependent upon subjective estimates by the investigator. Nevertheless, it is possible that a better measure of improvement might bear a higher relationship to EPS.

These findings on the predictive accuracy of the EPS cannot be compared directly with the original data of Wittman because of differences both in criteria of improvement and in mode of presentation of data. However, prognostic accuracy was clearly much higher in Wittman's study than in the present one. In both cases the schizophrenics and their social histories were those of the Illinois public hospital system, although about 20 years intervened between the two studies. Quite possibly, differences in patient population, in treatment procedures or in discharge rate account for some of the difference. Also, our sample was 62 per cent first admissions, and the remainder averaged only five months' previous hospitalization. Wittman's were unselected by number of previous admissions, and presumably were much more chronic. In our present sample, 50 per cent were discharged after nine months. Wittman used ratings of improvement and did not present discharge data. Also, she did not specify the interval between admission and follow-up. However, her follow-up period apparently was longer and her discharge rate less.

It is possible that Wittman was able to use the EPS more effectively because she, as the author of the scale, had a firmer grasp of the intent of the items. The present investigators had a good bit of difficulty using the scale in a meaningful manner. There were several reasons for this.

The wording of the descriptive labels which Wittman offered for items, although clinically rich and descriptive, is exceedingly imprecise. It is not entirely clear what is meant by "shut-in" personality or "poor bite on life." Even phrases like "withdrawal and disinterest in social surroundings" or "dates frequently" were found to be decidedly ambiguous in the absence of specification of the degree and variety of withdrawal, or how many dates. Ambiguity in some items results from a lack of specification of whether the material is to be rated at a behavioral or an interpretive level—*e.g.*, "insecurity and inferiority feelings" (Item L). Also, some of the items call for a judgment which is very difficult to rate objectively, even with optimal information —*e.g.*, "low-energy tone" (Item G).

Furthermore, in at least three items, two characteristics are stated as opposites which are not necessarily opposite. For example, in Item J the rater must score "marked academic interests vs. active interests in sports"; in Item K he must score "careless indifference vs. worrying, self-conscious type"; in Item L he must score "exclusive stubborn traits vs. insecurity and inferiority feelings." In the present sample several patients were both studious and athletic, or both self-conscious and carelessly indifferent, or stubborn with feelings of inferiority. Also, some patients showed neither of the two characteristics—*e.g.*, they were neither studious nor athletic.

Many single items contain a variety of characteristics not necessarily related, with the requirement that they all be rated on a single continuum. For example, we are asked to rate in Item O:

"*Shut-in personality. General:* The psychotic condition is simply an exaggeration of the peculiar type of personality shown all through childhood. Stormy childhood often with overprotection and anxiety, a difficult adolescence characterized by inability to get along with and mix with other children. Constitutional apparently rather than product of specific environment."

In addition, many of the items appear to

refer, at least in part, to the same variables. The descriptive paragraphs of nine of the 20 items mention, among other things, something about inadequate social interests.

These difficulties tend to reduce the certainty that the scores assigned by different investigators have the same meaning. It is true that Wittman reported a two-rater reliability of .87. In the present study, two clinicians rated 67 of the cases and obtained a reliability of .97. This, however, was obtained only through extensive discussion of earlier cases, and agreement as to the interpretation of the items. Presumably the same was true for Wittman and her coworker. A reliability coefficient obtained in this manner reflects more nearly the working relationship of the co-workers than characteristics of the scale as an instrument for general use.

In her original article, Wittman reported a marked bimodality of the scores (29, p. 22). She interpreted this as indicating that "two definitely separated classes or types have been included in the sampling, thus the hypothesis of Langfeldt—that there are two types of schizophrenia differentiated as to prognosis appears to be corroborated." The present sample also gave some hint of bimodality, as seen in the frequency distribution in Table 4. Becker (1) obtained a similar bimodality. This effect might be an artifact of a tendency of the raters to respond to the diffuseness of the items by rating them in accordance with an overall impression of whether the patient is process or reactive. The raters in the present study sometimes felt that they were doing this.

TABLE 4

Frequency Distribution of EPS Scores

Score	No. of *Ss*
Below 20	1
20–27	7
28–35	24
36–43	10
44–51	14
52–59	18
60–67	18
68–75	11
76–83	3

Despite all of its deficiencies, the scale does to some extent measure something that is meaningful about patients. In the present study, as in Wittman's investigations, a significant relationship was found between the EPS and outcome. Also, several investigators (*e.g.*, 2, 7, 10, 32) have obtained significant relationships of EPS to perceptual, cognitive and physiologic variables.

In approaching the problem of improving the EPS, one finds an ambiguity concerning what the EPS is intended to do. If the process-reactive distinction derives its meaning primarily from its prognostic value, then any scale measuring it might reasonably include all variables which predict outcome, irrespective of their congruence with the traditional descriptions of the process and reactive syndromes. For example, age and socioeconomic status have been reported to be related to outcome. Yet these variables do not make good sense as identifying features of subvarieties of schizophrenia. On the other hand, one might take the view that the scale is identifying two syndromes or subvarieties of schizophrenia which are useful to study, whether or not all of the symptoms have prognostic value. This view of the problem would seem to indicate that the scale should retain items which tend to correlate with one another, regardless of their individual prognostic values.

A difficulty with both approaches is that the prognostic relationships in question probably vary among clinical samples, as from rural to urban, from lower to upper socioeconomic groups, or with educational level, or sex of patient. Process schizophrenia is supposedly characterized by poor pre-morbid adjustment, but the signs of poor adjustment probably vary with the sociocultural subgroup from which the patient comes. This has been pointed out, in effect, for the case of males as compared to females by Farina *et al.* (6), who make

th point that, for females, marriage need no indicate the same degree of maturity as it does for males. However, the logic of their argument might be extended to ot er differences between subgroups of pa- ti nts as to ease of attaining various be- ha viors. For example, the Phillips Scale pl ces heavy emphasis on pre-morbid het- er sexual behavior, including sexual in- te course, as an identifying characteristic of he atypical (*i.e.*, reactive) schizophrenic. H wever, Kinsey *et al.* (11) found that the he erosexual behavior of males varies mark- ed y with educational level and socioeco- no nic status, the lower-level males showing much more heterosexual intercourse. Hence, for lower-level males, heterosexual activity need not imply the same degree of maturity as it does for middle-class males. One might ask if such groups truly differ on pre- morbid "adjustment." Is the typical middle- class youth less well adjusted than the more heterosexually active lower-class youth?

There is more direct evidence that score on the Phillips Scale varies with the pa- tient's subculture. Query (24) applied the Phillips Premorbid Scale to male schizo- phrenics both from the rural areas of Ken- tucky, where there is a subculture which differs considerably from the mainstream of American culture, and from Louisville and Cincinnati. She found that male schizo- phrenics from the rural area were much more heterosexually active before their ill- ness, and hence obtained higher scores on the Phillips Scale, than were comparable patients from the urban communities. Moreover, this difference on heterosexuality is exactly what one would expect for normal young men in these two subcultures. Query points out that the rural culture is much more patriarchal, which should make adop- tion of the male sexual role easier. (Un- fortunately, we do not yet know whether the "good pre-morbid" patients from the country showed better remission than the "poor pre-morbid" patients from the cities.) Query's findings show clearly that if a Kentucky investigator chose "good pre- morbid" and "poor pre-morbid" patients by the Phillips Scale, he probably would have groups which differed sharply on socio- cultural background. It therefore appears possible that other investigators who used the Phillips Scale for this purpose may have obtained groups which differed on socio- cultural backgrounds, and that some of the differences found on other measures be- tween these two groups may in part re- flect such disparity in background.

There are also items on the EPS which might be expected to have differing implica- tions for level of adjustment, depending upon the subculture—*e.g.*, Item E, range of interests; Item J, marked academic in- terests vs. active interests in sports; Item K, careless indifference vs. worrying, self- conscious type. A well adjusted young man of upper middle-class background would more likely have wide interests (a reactive sign) and academic interests (a process sign) than would a well adjusted member of a lower-class group. Also, "careless in- difference" is probably more socially ac- ceptable in some lower-class groups than in the middle class.

Perhaps we need separate prognostic scales for different groups of patients. A re- view of the literature on prognosis indicates that this may be so. The findings concern- ing prognostic variables have been highly conflicting. For example, a large number of studies like the present one have found that married patients have a better prognosis than do unmarried. This has been reported for men (4, 9, 14, 17), for women (6, 21), both for men and for women (15, 20), and for groups of patients consisting of both men and women (16, 18, 22, 31). However, one of the most thorough studies of all (27), using patients from the University of Min- nesota Hospitals, found no such relationship. What variables account for such discrepan- cies is not clear, but other characteristics of the patient population, such as sociocultural background, seem worth investigating as to

their effect on the prognostic value of the various predictors.

These considerations appear to raise important questions about the meaningfulness of the process-reactive distinction, since a given characteristic has a different implication for adjustment depending upon the population. Since at present, in selecting "process" and "reactive" schizophrenics from various populations, investigators use a single set of criteria without regard to their varying meaning, the various groups of "process" or "reactive" schizophrenics may not be comparable as to prognosis. Also, it seems to make little sense to hypothesize that such groups, from different populations but with similar scores on the EPS or on the Phillips Scale, are similar in the degree to which psychogenic or constitutional factors contribute to their illness. It would appear that process and reactive schizophrenias, if they exist, must have different behavioral manifestations in patients from different sociocultural backgrounds. This might account for the fact that Zuckerman and Grosz (32) and King (10) obtained diametrically opposite results when they compared process with reactive patients on the Funkenstein test.

Measures of socioeconomic status which are meaningful for research on process and reactive schizophrenia should be obtained on the parents of the patient, rather than on the patient himself. The usual indices—education, income, occupation and address—might be lowered more by a poor premorbid adjustment than by a good one. Obtaining such information on the parents would require considerable expenditure of research effort, since it is usually not available in the typical case history file. Such an investment of research time would appear to be justified, however, by the importance of this line of research. The findings of the Duke group (8) on differences betweeen patients with "good" and "poor" pre-morbid adjustment seem especially promising. This type of research may yet produce a major advance in our understanding of schizophrenia.

SUMMARY

The prognostic validity of the Elgin Prognostic Scale (EPS) was examined for 105 newly admitted patients. Each patient was rated on the EPS at the time of admission, and his discharge status was obtained nine months later. Total EPS score was significantly related to discharge status from the sixth through the ninth month after admission. At the ninth month (by which time 50 per cent of the patients had been discharged), the biserial correlation between EPS and discharge was .38, a statistically significant relationship ($p < .001$). However, only 11 of the 20 items had a significant relationship to discharge status.

The score on the EPS bore a marked relationship to marital status, married patients more often being classified on the reactive end and single patients on the process end. Marital status alone predicted discharge about as well as did the EPS.

The characteristics of good adjustment probably vary in different subcultural groups. If this is so, the process-reactive distinction should have different manifestations in the various populations; and the meaning of a score on a given scale based on pre-morbid adjustment, as well as its prognostic significance, should fluctuate with the sample. It follows that, to identify process or reactive patients, one needs different scales for patients from different subcultural groups.

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THE SIGNIFICANCE OF THE BASAL GANGLIA IN CLINICAL NEUROLOGY

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Wisdom begins when we realize that
we know nothing.

SOCRATES

The basal ganglia are traditionally associated with the "extrapyramidal motor system." The concept of this latter has always been somewhat vague; closer examination of it in anatomic and physiologic terms serves only to make confusion worse confused. By definition, the expression should include all those motor systems the fibers of which do not pass through the medullary pyramids (omitting those corticobulbar fibers which are strictly analogous to the corticospinal fibers of the pyramidal tracts). This would include those fibers with impulses affecting the function of glands, the contraction of smooth muscle, and the general control of the internal milieu (hypothalamic efferent system); and those fibers with impulses ultimately causing changes not only in autonomically controlled organs, but also in the skeletal musculature, in response to emotion (rhinencephalic efferent system). Yet the term "extrapyramidal motor system" is commonly used to mean only those neurons which control tone and postural activity in skeletal muscle. Bucy (1) has questioned the existence of a "pyramidal tract" in functional terms, but anatomically it certainly exists, however wrong earlier concepts of its function may have been. Undoubtedly the term "extrapyramidal motor system," as currently used, is both anatomically and functionally meaningless. Insofar as we are dealing with that system which controls tone and postural activity in skeletal muscle, it may be justifiable to retain the expression (though "nonpyramidal" might be better)—with the proviso that it is in fact only one of several such systems.

Clinicopathologic and, more recently, surgical experience has shown that the basal ganglia are the nub of this system. It must be admitted at once that, compared with some other grey masses in the brain, very little is known of the connections and functions of these nuclei. They are, as Paracelsus said of the spleen, "organum plenum mysterium"; anything that may be said of them in the present state of our knowledge must be largely speculative. Yet speculation may be useful insofar as it induces others to speculate also. Moreover, the amount of apparently unconnected information scattered throughout the clinical, pathologic, surgical and experimental literature justifies an attempt at integration, even in the present incomplete state of knowledge.

The first fact that must be faced is one which has been known for over a half-century: that the forebrain (probably exclusive of the cortex) is developed from the alar, or sensory, lamina of the primitive neural tube. Thus the structures within it, of which the neostriatum (NS)—*i.e.*, the putamen and caudate nucleus—is the most important, cannot, by definition, have a direct motor function. Years of research have failed to produce evidence of direct connections from the NS to lower motor centers in the brain stem or cord; it has perhaps been insufficiently realized that, if they were found, the carefully built up neurologic theories of almost 100 years would have to go by the board. The best we can hope of the NS in terms of motor function is that it forms an important link in a feedback circuit which ultimately controls true motor centers.

The paleostriatum (globus pallidus, GP),

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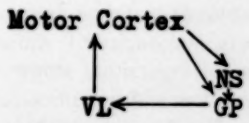
on the other hand, may be, in part, developed from the primitive ventral thalamus of the diencephalon (12). In adult anatomy, this becomes the subthalamus, zona incerta, and perhaps the hypothalamus. The GP is in fact anatomically continuous with the substantia nigra (SN) of the midbrain, and the cells it contains are of the large type commonly found in true motor structures. These considerations lend credence to the possibility that the GP may be, at least partially, a true motor center. It is the purpose of this paper to trace the feedback circuits in which the basal ganglia are involved and the efferent projections of the motor part of the GP, and to show, as far as is at present possible, how they are associated with "extrapyramidal" function.

As Gooddy (4) has pointed out, even the cortex should be considered as a point in a loop, rather than as a supreme center which receives, integrates, stores and emits messages. However, as long as we continue to consider "systems" as separate entities, they must be given a start at some fixed point, and the cortex is the obvious place.

Although different functional areas of the cortex have long been recognized, the idea is only slowly emerging that the physiologic properties of any given area of cortex are a function, not of its response to electrical stimulation (*i.e.*, its efferent connections), or of its cyto-architectonic structure, but of its thalamocortical connections. Most of the neocortex may be regarded as an umbrella erected on the thalamus, each separate functional area receiving a point-to-point projection from a specific thalamic nucleus. Phylogenetic studies show that the neocortex and thalamus (and the NS) develop *pari passu* with one another. The "motor cortex," in the broader (and correct) sense, may be defined as the projection area of the ventrolateral nucleus of the thalamus (VL); this corresponds roughly to areas 4, 6 and 8.

The next logical step, therefore, is to consider the afferent connections of VL. It is well known that it receives fibers from the dentatorubrothalamic tract (brachium conjunctivum), originating in the cerebellum (Cb). The fact that neurosurgeons have recently found lesions of VL to be as effective as those of GP in the symptomatic relief of Parkinsonism serves to emphasize that VL also receives an important afferent contribution from GP. This has been known for some time, but that it is as functionally significant as the contribution from the Cb has perhaps been insufficiently appreciated hitherto. As far as is at present known, Cb and GP are the only extrathalamic sources of afferent supply to VL.

Tracing these neuron pathways further back, we find that the Cb in its turn receives its afferents from three principal sources: 1) the peripheral proprioceptor mechanism, 2) the olive, and 3) the pontine nuclei. As the latter are functionally concerned with the true pyramidal motor system, they will not be further considered here. The origins of afferents to GP are not fully known, but we are already cognizant of a large and bewildering number of sources. For the purposes of the present discussion, however, only the five most important and well established need be considered: 1) the motor cortex, 2) the NS, 3) the substantia nigra (SN), 4) the brachium conjunctivum (from Cb), and 5) the subthalamus. Since NS itself is also known to receive afferents from the motor cortex, it can be seen that a short loop is already established:[2]



[2] Key to symbols in this and succeeding figures:

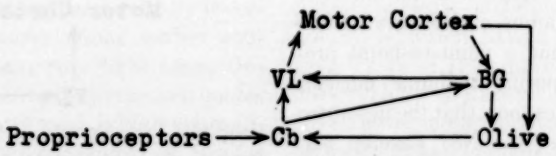
VL:	ventrolateral nucleus of thalamus
GP:	globus pallidus
NS:	neostriatum (putamen and caudate nucleus)
SN:	substantia nigra
LMN:	lower motor neuron
Cb:	cerebellum
MRC:	motor reticular centers of medulla
Sth:	subthalamus
ITN:	intralaminar nuclei of thalamus

Since NS and GP together make up the basal ganglia (BG), the figure can be simplified to:

It will be shown that other and more complex loops can be superimposed upon this, and an attempt will be made to explain their functional significance. But it should be stressed that we have already demonstrated a simple feedback circuit whereby there is mutual interaction between the motor cortex and basal ganglia.

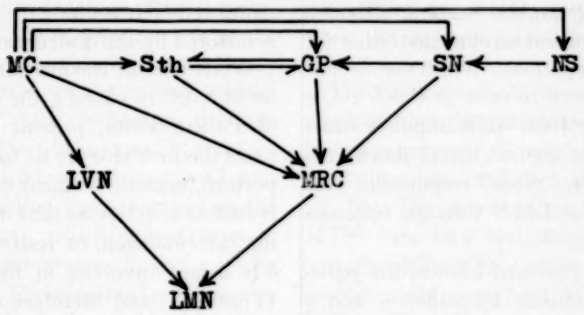
The next loop to be considered operates through the cerebellum. As far as the "extrapyramidal" motor system under consideration is concerned, the essential link in this chain is the olive. This structure receives afferents from the motor cortex, the NS, the GP, the red nucleus and, indeed, all the nuclei associated with this non-pyramidal motor system (15). In its most simplified form, this loop, in isolation, may be expressed:

Here, again, we have a simple figure of three mutually interacting structures. It may be integrated thus with the second figure above:

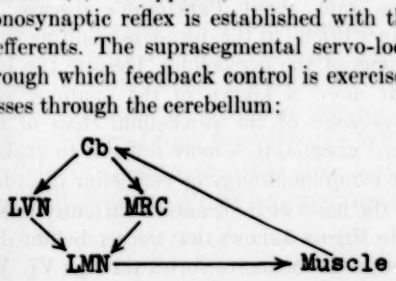


As shown by the figure, the cerebellum also receives proprioceptive afferent information *via* the spinocerebellar and (putative) trigeminocerebellar tracts from the spindle organs in the skeletal muscles, which are, of course, the ultimate effectors of the whole system under consideration. Because of this, the input and output of both of these interrelated circuits, instead of being self-maintaining, are constantly being modified and modulated by the afferent proprioceptive information so supplied.

Although there are further loops to be considered, it would be convenient at this stage to break off and pay attention to the mechanisms whereby efferent influences are brought to bear on the lower motor neuron (LMN), so that a theoretical basis may be given for certain clinical observations. The known non-pyramidal motor pathways immediately above the LMN are three in number; 1) rubrospinal, 2) (lateral) vestibulospinal (deiterospinal), and 3) reticulospinal. The importance of the first of these is a matter of some contention, but the consensus is that it is of less significance in man and other primates than in the cat. The second is to some extent an independent system, though it is tied to the others to a certain degree because it is under the influence of the motor cortex, and joins in the feedback loop through the cerebellum. The third may therefore be considered to be the most important from the present point of view. The actual neuron pathways by which the cells of origin of the reticulospinal tract are brought under higher (*i.e.*, more cephalic) influences are at present ill understood, but if we leave out the red nucleus and its (possible or probable) downward projections, and the intermediate connections between the midbrain and medullary motor reticular centers (MRC), the connections may be summarized as follows (MC: motor cortex; Sth: subthalamus; LVN: Deiters' (lateral vestibular) nucleus):



Researches initiated by Leksell and Granit have shown the LMN to be in fact twofold. The α motor neurons supply extrafusal muscle fibers, while the smaller γ motor neurons innervate the intrafusal fibers in which the proprioceptors are situated. To obtain smooth postural tone or contraction of a muscle, the discharge of α and γ motor neurons must be closely integrated and synchronized. Some of this integration occurs through monosynaptic (spinal) segmental reflex arcs, whereby afferents from the (γ-driven) intrafusal muscle spindles establish synaptic contact with the α motor neurons (6). It appears, however, that no monosynaptic reflex is established with the γ efferents. The suprasegmental servo-loop through which feedback control is exercised passes through the cerebellum:



To attempt to make a composite figure involving all of the preceding would only be confusing; but the subtle reader who bears them all in mind will instantly perceive that interruption at any point in this complicated circuitry will produce symptoms of disordered motor activity. It is well known that lesions in the cerebellum cause "intention tremor," whereas Parkinson's disease, in which the basic pathology is thought to be in the substantia nigra, is essentially characterized by "tremor at rest," which tends to diminish or disappear when voluntary movement is performed. It was classically taught that these symptoms are, as it were, equal and opposite, or mutually exclusive from a differential diagnostic point of view. But since, with the advent of surgical measures for the relief of Parkinsonism, this syndrome has been more carefully studied, the distinction has become less clear. Moveover, in diseases grouped under the heading of "olivopontocerebellar degeneration," a mixed symptomatology of both types may be seen.

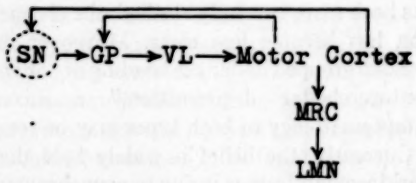
Currently, the belief is widely held that Parkinsonian tremor is due to asynchronous discharge of α and γ motor neurons. But it is also known that the tremor is pyramidally mediated, in that it can be abolished by section of the corticospinal (pyramidal) tracts —although at the expense of the ability to perform skilled movements. This shows that normal synchronous discharge of α and γ LMNs must require integrated activity of both pyramidal and non-pyramidal upper motor neurons. On anatomic grounds, the only place where the integration of the two systems can occur is in the motor cortex.

The work of Granit and Kaada (5) has indicated that the α and γ components of the LMN are differentially driven from the medullary motor reticular centers—*i.e.*, that there are probably separate and independent reticulospinal neurons governing the activity of α and γ efferents, respectively. It is tempting to suggest that these reticular neurons may, in their turn, be differentially driven

by higher non-pyramidal centers, although there is at the moment no evidence either for or against this hypothesis. Whatever mechanism may be shown to exist at this level, it seems safe to say that, while impulses causing Parkinsonian tremor travel down corticospinal fibers, those responsible for rigidity reach the LMN through reticulospinal pathways.

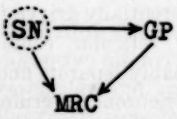
As far as is at present known, the pathways and mechanisms by which α and γ synchrony are controlled from higher centers have been described. It is reasonable to suppose that the motor cortex, acting on information received from VL, "sets" the rate of discharge of the LMN by means of its integrative action. Lesions of the SN may upset this balance in one or both of two ways.

1) A lesion in the circuit:



may disturb the input to the motor cortex so that its subsequent output to motor reticular centers results in asynchronous discharge to the LMN. This theory demands that pathologic changes in the SN cause it, not to go out of action, but to discharge abnormal impulses through GP and VL to the motor cortex; therefore, surgical lesions in either GP or VL cut off these abnormal signals and, as it were, restore the balance.

2) In addition to their upward impulses, both SN and GP (but not VL) discharge downward to motor reticular centers, through a circuit which may be represented:



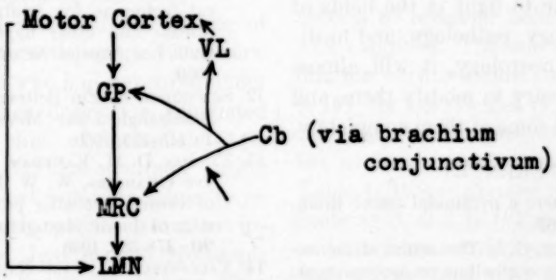
In this case, lesions of SN, whether destructive or causing abnormal discharges, would cause an "out of step" input to the motor reticular centers, the balance of which is restored by the destruction of GP.

While both of the mechanisms above may be at work in causing the symptomatology of Parkinsonism, present experience indicates the first (Figure 6) to be the more important, because surgical destruction of VL is just as effective as that of GP in alleviating the condition, or restoring the balance. VL is not involved in the second circuit (Figure 7), and therefore symptomatic relief caused by its destruction cannot be accounted for by this mechanism.

The recent surgical experience of Rand (11), whereby destruction of SN alleviates Parkinsonian symptoms, lends color to the theory that the syndrome is caused by abnormal discharges from SN, rather than by destructive pathology. So, indirectly, does the observation that some patients whose symptoms have been relieved by pallidotomy may subsequently deteriorate. Once the GP has been destroyed, only a progressive increase in abnormal discharges from SN could further upset the balance.

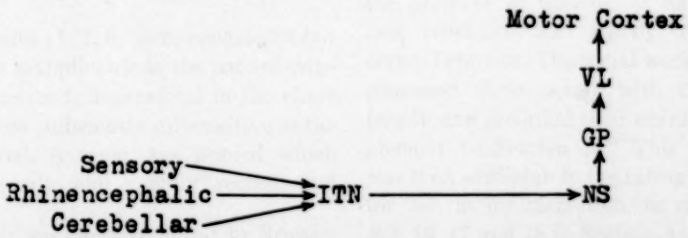
It is interesting, in the light of what has been said about Parkinson's disease, to glance briefly at the disorders resulting from lesions of the cerebellum. Despite the fact that more is known of the anatomy and physiology of the cerebellum than of the basal ganglia, it is more difficult to explain the symptomatology of cerebellar disorders on the basis of information currently available. Figure 3 shows that the cerebellum discharges to the motor cortex through VL. Yet surgical destruction of this nucleus does not produce any permanent symptoms referable to the cerebellum. Another important component of the brachium conjunctivum goes to GP, yet even combined ipsilateral lesions of GP and VL have no effect in this direction. One might be tempted to implicate the connections from the cerebellum to the medullary motor reticular centers, which pass through the inferior peduncle, and have been shown to affect the activity of α discharge from the LMN (2). But it is known that

w en the inferior peduncle is intact, experim ntal lesions of the dentate nucleus or the su erior peduncle (brachium conjunctivum) b ow its decussation in the midbrain cause in ention tremor. As it decussates, the brach um conjunctivum bifurcates into an asce ding limb, which is distributed to VL and G , and a descending limb (arrowed in the fo lowing diagram), which passes down to th motor reticular centers in the medulla (a d beyond?). Experimental evidence in th monkey (9) shows that isolated destruction of the descending limb alone, like isolated destruction of the ascending limb (GP and VL), does not cause the appearance of symptoms of cerebellar damage.



Since, in the case of cerebellar disease, we are dealing with a known destructive lesion, it must be assumed that when direct Cb influence (through the brachium conjunctivum) is removed from *both* GP *and* MRC, the balance of input to MRC from higher centers (including GP) is disturbed, resulting in asynchronous discharge to LMN. That the tremor appears only on movement is explained by the fact that the afferent input to Cb comes from the muscle spindle organs; therefore, the modulating influence of the cerebellum on motor output occurs only when the cerebellum itself is excited as a result of movement.

The final circuit to be considered is dependent upon the point-to-point projection from the intralaminar nuclei of the thalamus to the caudate nucleus and putamen (NS) (8, 10). The intralaminar thalamic nuclei (ITN) are now well known for the important role they play in the reticular arousal system of Moruzzi and Magoun (7). The ITN receive reputedly nonspecific impulses from many sources, of which the best known are the peripheral special and somatic sensory receptors. However, Valenstein and Nauta (14) recently emphasized the projection from the rhinencephalon to these nuclei in a series of animals, and, in the cat at least, they also receive fibers from the cerebellum (3, 13, 16). It can be shown that these nonspecific signals may influence the output of the motor cortex in the following way:



It is perhaps by this pathway that increased muscle tone is brought about by peripheral afferent stimulation (*e.g.*, pain). Emotional states are believed to be accompanied by increased rhinencephalic dis-

charges, and they are known to be accompanied by changes in muscle tone. It would appear that psychogenic hypertonia is brought about by increased rhinencephalic (hippocampal) bombardment of the ITN through the mechanism illustrated above. Finally, if the same, or similar, connections obtain in man as in the cat, it may be assumed that the hypotonia observed in destructive lesions of the cerebellum is due to the diminution of the number of afferent impulses arriving at the NS and motor cortex *via* the ITN.

These thoughts on some of the functions of the basal ganglia are tentative conclusions, based on data known to be incomplete. As new facts come to light in the fields of anatomy, physiology, pathology, and medical and surgical neurology, it will almost certainly be necessary to modify them, and perhaps to discard some of them completely.

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OBJECTIVE EVALUATION OF PSYCHOTIC PATIENTS UNDER DRUG THERAPY: A SYMPTOM AND ADJUSTMENT INDEX[1]

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In recent years there has been a remarkable increase in the number of previously psychotic patients under maintenance drug treatment in outpatient clinics. This paper reports the development of a new psychiatric index specifically designed for rating psychiatric outpatients in total or partial remission from psychotic illnesses.

A double-blind study of the effects of withdrawal of ataractic drugs in chronic psychotic patients who had improved under treatment with such drugs was conducted in the outpatient clinic of Springfield State Hospital (3, 4). Various available rating scales were reviewed but were found to be inapplicable to the specific situation involved. The Lorr, Jenkins and Holsopple Multidimensional Scale for Rating Psychiatric Patients (7) was devised for hospitalized patients. Another scale by Lorr *et al.* (8), while devised for outpatients, was found to be concerned largely with psychoneurotic symptoms and thus of limited usefulness in a population with a high incidence of residual and recurring psychotic symptoms.

Other scales (1, 2, 5) were considered but found to be inapplicable to the patient population concerned, impractical in the clinic setting, or not sufficiently informative in the areas desired. A scale was needed which could be easily and rapidly scored, and which would reflect changes in behavior, in psychotic or neurotic symptomatology, and in social adjustment.

CONSTRUCTION OF INDEX

Since no instrument both satisfactory and readily usable was available, one was devised for the study. A list of symptoms and aspects of behavior believed to be ratable was compiled. The sources for this compilation were standard psychiatric texts (6, 9, 10), the Lorr Scales (7, 8) and, of course, the clinical experience of the authors. The list of items finally selected is shown in Table 1. It was divided into three sections. Sections A and B ("Psychotic Symptoms" and "Psychoneurotic Symptoms") were designed to be filled in by a rater with experience in psychiatry or clinical psychology after an interview with the patient. Section C ("Social Adjustment") was designed to be rated by a social worker on the basis of information supplied by the patient's family. It was felt that the short patient-psychiatrist interview might not always reveal the presence or absence of hallucinations, rage reactions, and grossly inappropriate sexual behavior. The social worker therefore discussed these points with the patient's family and recorded their opinion on "Supplement to Section A." This information was then available to the rating psychiatrist for use, at his discretion, to modify items #2, 16, 17 and 18 in Section A.

It was found most convenient to establish three degrees of severity for each item, with an additional column for "not present" or "minimal" and one for "unratable" (corresponding roughly to this scale: 0, absent;

[1] This study was supported in part by Research Grant #MY-2147 (R-1) and MY-3672 from the National Institutes of Health, U. S. Public Health Service.

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TABLE 1

Springfield Outpatient Symptom and Adjustment Index

Section A Psychotic Symptoms

Date: Patient's Name:
Number: Examiner (Psychiatrist or Psychologist):

Symptom	0	1	2	3	U	Inter-rater Reliability Coefficient	p
1. Hallucinations, attitude toward						0.78	*
2. Hallucinations, frequency of						0.89	*
3. Delusions, plausibility of						0.93	*
4. Delusions, frequency of						0.78	*
5. Ideas of reference						0.69	*
6. Orientation						0.98	*
7. Hypoactivity						0.50	*
8. Hyperactivity						0.64	*
9. Manneristic behavior						†	
10. Speech, incoherence and irrelevance						0.93	*
11. Speech, decreased productivity						0.82	*
12. Speech, increased productivity						0.71	*
13. Speech, stereotyped or neologistic						0.76	*
14. Depression						0.88	*
15. Elation						0.00	1.00
16. Irrational rage reaction						0.96	*
17. Frequency of such rage reactions						0.69	*
18. Grossly inappropriate sexual behavior						†	
19. Suicidal thoughts						†	
20. Attitude toward hospitalization						0.88	*
21. Flattening of affect						0.78	*
22. Other regressive phenomena						0.74	*

Score ———

TABLE 1—*Continued*

Section B Psychoneurotic Symptoms

Date: Patient's Name:
Number: Examiner (Psychiatrist or Psychologist):

Symptom	0	1	2	3	U	Inter-rater Reliability Coefficient	*p*
24. Feeling of inadequacy						0.68	*
25. Minor speech defect						0.67	*
26. Feelings of self-consciousness						0.77	*
27. Self-criticism						0.81	*
28. Tendency to blame others						0.62	*
29. Disturbance by hostile impulses (not acted out)						0.74	*
30. Hostile attitude						0.79	*
31. Troubled by feelings of guilt						0.92	*
32. Obsessions						0.69	*
33. Compulsive acts						0.33	0.02
34. Compulsive acts, frequency						0.33	0.02
35. Feelings of anxiety						0.72	*
36. Disruption of activity by phobia						0.00	1.00
37. Tension, as evident to observer						0.47	*
38. Somatic complaints, headaches						0.74	*
39. Somatic complaints, fatigue						0.72	*
40. Somatic complaints, "heart"						0.00	1.00
41. Somatic complaints, gastrointestinal						0.52	*
42. Somatic complaints, other (specify)						0.79	*
43. Concern over sexual problems						0.88	*
44. Frequency of concern over sexual problems						0.94	*
45. Disturbance of sleep, insomnia and/or restlessness						0.74	*

Score _____

TABLE 1—*Continued*

Section C Social Adjustment

Date: Patient's Name: Number: Examiner (Social Worker or Public Health Nurse): Symptom	0	1	2	3	U	Inter-rater Reliability Coefficient	*p*
45. Eating habits						0.44	0.002
46. Overeating						0.78	*
47. Undereating						0.50	*
48. Sleeping habits						0.84	*
49. Drinking habits						0.93	*
50. Smoking habits						0.84	*
51. Dress						0.87	*
52. Suppression of aggressive impulses						0.73	*
53. Degree of hostility						Not rated	
54. Mood, elated						0.00	1.00
55. Mood, depressed						0.83	*
56. Excitability						0.71	*
57. Empathy						0.63	*
58. Ambition						0.56	*
59. Adaptability to change						0.71	*
60. Degree of social activity						0.62	*
61. Degree of responsibility at home						0.61	*
62. Capacity for employment						0.84	*

Score _____

Supplement to Section A

The scores of the ratings below are to be used by the rater of Section A to modify—if indicated—the scores for items 2, 16, 17 and 18.

A2-a Talking to self						0.87	*
A2-b Responding to voices						0.89	*
A 16 Irrational rage reaction						−0.07	0.62
A 17 Frequency of such rage reactions						−0.04	0.78
A 18 Grossly inappropriate sexual behavior						†	

* $p = <0.001$. † Not observed in patient sample.

1 mild; 2, moderate; 3, severe; U, unratable). Detailed definitions were worked out for each degree of each item.[4]

The authors were faced with the problem of whether the various items of the index should be differentially weighted. An attempt to reach a consensus among several experienced clinicians as to what weights should be assigned to the various symptoms was unsuccessful. Under these conditions, use of a weighting system would be expected to introduce errors exceeding those inherent in a non-weighted scale. It seemed obvious that patients showing "severe" symptoms which would have been heavily weighted would have so many additional symptoms that they would receive a high score whether items were weighted or not. (In practice, this was found to be the case.)

Among available instruments, the Lorr Scale for in-patients comes closest in content to the Springfield index. It should be noted, however, that the Lorr Scale divisions of severity vary from three possible degrees to six, whereas the Springfield index items, with one exception (#50), are all scaled in three steps. Some items on the Lorr Scale (*e.g.*, "mood") are ratable in two directions from a "normal" value (for "mood," toward elation or depression). On the index this does not occur: any deviation from "normal" has a "positive" value (*e.g.*, two items relate to mood—#14 to "depression," #15 to "elation"). This makes possible a simplified quantitative score.

Approximately a third of the 62 items (1, 2, 3, 5, 9, 13, 19, 23, 25, 26, 27, 30, 32, 36, 37, 38, 39, 44) are essentially the same as the ones on the Lorr Scales. Another third (6, 7, 8, 10, 11, 12, 14, 15, 16, 21, 28, 29, 31, 34, 40, 42, 45, 46, 47, 48, 52, 53, 54, 55, 56) are similar to but not identical with Lorr Scale items. The remaining third (4, 17, 18, 20, 22, 24, 33, 35, 41, 43, 49, 50, 51, 57, 58, 59, 60, 61, 62) are items not included in the Lorr Scales.

[4] Definitions used in this index may be obtained from the authors on request. The following are samples for each of the Sections A, B and C: *Hallucinations, attitude toward:* 0, none; 1, doubts them; 2, certain they exist; 3, obeys them. *Compulsive acts:* 0, none; 1, resists with little discomfort; 2, resists only with considerable discomfort; 3, unable to resist. *Dress:* 0, keeps self neat and clean; 1, careless and indifferent in grooming; 2, requires supervision; 3, has to be washed and/or dressed.

RATER RELIABILITY

To investigate the inter-rater reliability that could be obtained with the index, two examiners rated 50 patients on each of the sections. The ratings on Sections A and B were made by a psychiatrist and a psychologist after a clinical interview with the patient; those on Section C, by two social workers after an interview with the family or other persons with whom the patient was living. In each instance both raters were present during the interview but one took the lead in conducting it. Ratings were made independently, after the completion of the interview.

The agreement between raters was computed for each item, using the intraclass correlation coefficient. Items not meeting our criteria of reliability were revised and re-rated. The coefficients of the final version are reported in Table 1. Of the items in Section A (Psychotic), only one (15, *Elation*) had a coefficient of less than .50. One (7, *Hypoactivity*) fell between .50 and .60. Three items (9, *Manneristic behavior*; 18, *Grossly inappropriate sexual behavior*; 19, *Suicidal thoughts*) were not observed in the patient sample rated. All of the others had reliability coefficients above .60.

Of the items in Section B (Psychoneurotic), five had coefficients below .50 (32, *Compulsive acts*; 33, *Compulsive acts, frequency*; 35, *Disruption of activity by phobia*; 36, *Tension, as evident to observer*; 39, *Somatic complaints, "heart"*). One item (40, *Somatic complaints, "gastrointestinal"*) had a coefficient between .50 and .60. Two of these items, those relating to compulsive acts, had low coefficients; they were rarely observed in the patient sample. The low reliability of two of the somatic complaint

items might result from the fact that only one of the two raters had had medical training.

In Section C (Social Adjustment), two items (45, *Eating habits*; 54, *Mood, elated*) had coefficients below .50, two (47, *Under-eating*; 58, *Ambition*) coefficients falling between .50 and .60. The two negative coefficients (A16 and A17) occurred on the "Supplement" only and did not influence the correlations of the corresponding items (16 and 17) on Section A itself, which had high coefficients (.96 and .69, respectively).

USE OF THE INDEX

The double-blind study for which the index was constructed was designed in the following manner:

From recently discharged and paroled patients, 144 were selected who had experienced a non-organic chronic psychosis, had improved under treatment with ataractic drugs, and seemed reasonably stabilized on medication.

Of the 144 patients, 56 were men and 88 were women; they ranged in age from 19 to 66, with a median age of 43.5. One hundred twenty-seven had a diagnosis of schizophrenic reactions (64 paranoid type, 42 catatonic, 21 other); 12 suffered from various types of depression, three from manic depressive illness, and two from psychosis with mental deficiency. Length of hospitalization ranged from two months to 35 years, with a median of 30.5 months. The patients had received a variety of treatments, but drug therapy appeared to be an important factor in recent improvement in all of them. Of the 144 patients, 92 were receiving chlorpromazine, 16 reserpine, 13 promazine, 12 prochlorperazine, and 11 perphenazine.

After a preliminary phase (average length, 4.3 months), during which the patients were evaluated and placed on their usual medication in unidentifiable capsule form, they were randomly distributed into either control or withdrawal groups. The medication was continued in the contro group. For patients in the withdrawal grou capsules containing the full maintenanc dose were gradually replaced by capsule containing progressively diminishing dose and, finally, inert placebos. After varyin lengths of time, the patients in the contro group were also withdrawn from medication so that eventually all patients were with drawn. The occurrence of relapse wa determined clinically by the treating psychiatrist. Withdrawal from ataractic medication markedly increased the likelihood o relapse. The average monthly relapse rat was four per cent for patients under full medication ("control group") and 12.3 per cent for patients receiving reduced medication or placebo.

Index ratings were made at three-month intervals (at least twice prior to withdrawal) until the time of relapse or until the termination of the study. Ratings were made by the treating psychiatrist (Sections A and B) and by the social worker (Section C).

Scores were determined in the following way: Each item checked in the first column (see Table 1) was scored as 0, in the second as 1, in the third as 2, in the fourth as 3, giving a numerical score for each section of the index and a total index score (the sum of the scores of the three sections). The scores on the Supplement to Section A were not counted as separate items but were used to modify the corresponding items on Section A as described above. Several different methods of handling the relatively infrequent unratable items were considered, but none seemed entirely satisfactory. It was discovered that all patients with more than one or two items so checked (largely mute or unco-operative patients) had a high score even if the scores were not prorated in any way. In the interest of practicality and simplicity, these unratable items were entirely disregarded in the scoring.

TABLE 2

Frequency Distribution of Total Index Scores (Initial and Final Ratings)

Total Index Score	Relapsed Group (N = 100)				Non-Relapsed Group (N = 20)			
	Initial (N)	%	Final (N)	%	Initial (N)	%	Final (N)	%
0–7	11	11.0	0	0.0	2	10.0	5	25.0
8–15	17	17.0	2	2.0	9	45.0	6	30.0
16–23	27	27.0	4	4.0	4	20.0	4	20.0
24–31	19	19.0	12	12.0	4	20.0	2	10.0
32–39	19	19.0	30	30.0	0	0.0	3	15.0
40–47	4	4.0	26	26.0	1	5.0	0	0.0
48–55	2	2.0	15	15.0	0	0.0	0	0.0
56–63	1	1.0	6	6.0	0	0.0	0	0.0
64–71	0	0.0	3	3.0	0	0.0	0	0.0
72–79	0	0.0	2	2.0	0	0.0	0	0.0
Total	100		100		20		20	
Median	21		40		14		14	

RESULTS

The data from only 120 patients were used as the basis of this evaluation of the index. Twenty-four cases were eliminated because of incomplete index data or because they were returned to the hospital by the families although the treating psychiatrist did not observe evidence of relapse. Of these 120 patients, 100 had relapsed and 20 had not when the study was terminated.

The distribution of the total index scores of the 120 patients is reported in Table 2. A number of the patients, in both relapsed and non-relapsed groups, received high initial scores; these were, of course, patients who were still quite ill at the time of the initial rating.[5] The distribution of total scores of the relapsed patients at the time of relapse (final scores) should be compared with the distribution of the final ratings of the non-relapsed patients. The general increase in the index scores at the time of relapse is apparent from this tabulation, as is the stability of the index scores in non-relapsed patients. In Figure 1 the initial scores are plotted against the final scores for all relapsed patients. It is seen that most of the scores of relapsed patients are above the "no change" line, indicating the general increase of index scores in these patients. Figure 2 shows the initial and final scores of the 20 patients who did not relapse; these scores are distributed around the "no change" line.

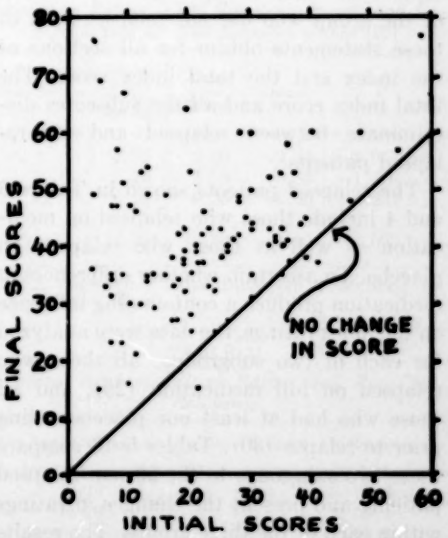

FIG. 1. Scattergram of initial and final scores of relapsed patients.

[5] In point of fact, the earliest ratings are not reported here, many having been made before revision of the index was completed. "Initial" ratings, herein shown, were made after the index was revised and pre-tested, and after the procedures of the investigation were well established.

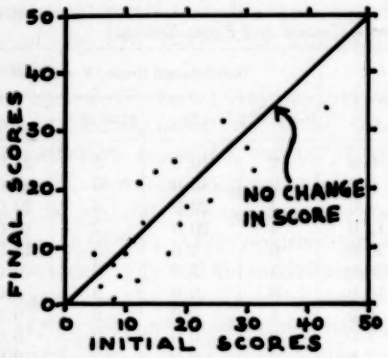

FIG. 2. Scattergram of initial and final scores of non-relapsed patients.

The results of a statistical analysis of data obtained from these 120 patients are reported in Tables 3 and 4. Table 3 shows that there are no significant differences between the initial scores of patients who later relapsed at any time during the study and the scores of those who did not relapse. Thus, at the beginning of the study the two groups were comparable. Table 4 indicates that the final scores of the group who relapsed are significantly higher than those of the group who did not relapse. Both of these statements obtain for all sections of the index and the total index score. The total index score and all the subscores discriminate between relapsed and non-relapsed patients.

The relapsed patients shown in Tables 3 and 4 include those who relapsed on medication as well as those who relapsed on placebo. To ascertain whether differences in medication produce a confounding influence on the index ratings, the data were analyzed for each of two subgroups: all those who relapsed on full medication (25), and all those who had at least one placebo rating prior to relapse (30). Tables 5–10 compare these two subgroups to the 20 non-relapsed patients and present the changes in ratings within each of the three groups. The results summarized in these tables indicate that the validity of the index was not affected, whether patients were receiving active medication or placebo at the time of relapse.

It would appear, then, that the index is a satisfactory indicator of relapse. As patients relapse, all the subscores and the total index score rise to a statistically significant degree. The scores of patients who do not relapse do not rise and, indeed, may fall slightly. As shown in Table 11, the initial total score and the percentage of patients who relapsed are positively related. Although this trend does not reach statistical significance, it is present in all phases of the study.

FACTOR ANALYSIS

A factor analysis of 23 items with reliability coefficients of .78 or better was computed by the principal axis technique. (By error, #4 was left out.) Ratings made while

TABLE 3

Initial Ratings and Significance of Their Difference for Non-Relapsed Group (N = 20) and Relapsed Group (N = 100)

Index Section	Mean Initial Rating (Non-Relapsed Group)	Mean Initial Rating (Relapsed Group)	Difference	
A	2.20	4.14	1.94	1.86 n.s.
B	5.20	7.72	2.52	1.89 n.s.
C	9.45	10.90	1.45	0.97 n.s.
Total	16.85	22.76	5.91	1.98 n.s.

TABLE 4

Final Ratings and Significance of Their Difference for Non-Relapsed Group (N = 20) and Relapsed Group (N = 100)

Index Section	Mean Final Rating (Non-Relapsed Group)	Mean Final Rating (Relapsed Group)	Difference	*t*
A	2.00	9.79	7.79	4.45*
B	4.25	15.61	11.36	6.46*
C	9.55	16.31	6.76	4.35*
Total	15.80	41.71	26.91	8.51*

* $<.001$.

TABLE 5

First Placebo Ratings and Significance of Their Difference for 20 Non-Relapsed Patients and Those 30 Relapsed Patients Having at Least Two Placebo Ratings

Index Section	Mean First Placebo Rating (Non-Relapsed Group)	Mean First Placebo Rating (Relapsed Group)	Difference	
A	2.35	3.27	0.92	0.840 n.s.
B	5.10	6.27	1.17	0.806 n.s.
C	9.75	10.53	0.78	0.440 n.s.
Total	17.20	20.07	2.87	0.857 n.s.

TABLE 6

Final Ratings and Significance of Their Difference for 20 Non-Relapsed Patients and 30 Relapsed Patients Having at Least Two Placebo Ratings

Index Section	Mean Final Rating (Non-Relapsed Group)	Mean Final Rating (Relapsed Group)	Difference	t
A	2.00	10.13	8.13	4.131*
B	4.25	13.80	9.55	4.951*
C	9.55	16.77	7.22	4.190*
Total	15.80	40.70	24.90	6.975*

* $<.001$.

TABLE 7

Change in Rating and Its Significance for Those 30 Relapsed Patients for Whom a Prior Placebo Rating was Available

Index Section	Mean First Placebo Rating	Mean Final Rating	Change	t
A	3.27	10.13	6.86	4.478*
B	6.27	13.80	7.53	7.004*
C	10.53	16.77	6.24	5.497*
Total	20.07	40.70	20.63	7.773*

* $<.001$.

the patients were still on medication and in relatively good condition were used. Table 12 presents the results of this analysis. The columns headed 1 through 7 contain the loadings for each of the seven factors derived from the analysis. Only the first two

TABLE 8

Change in Rating and Its Significance for 25 Patients Who Relapsed on Full Medication

Index Section	Mean Initial Rating	Mean Final Rating	Change	t
A	4.16	8.20	4.04	4.412†
B	6.96	14.76	7.80	6.765†
C	11.76	15.88	4.12	3.397*
Total	22.88	38.84	15.96	8.155†

* $<.01$.
† $<.001$.

TABLE 9

Change in Rating (Initial to Final) and Its Significance for Non-Relapsed Group (20)

Index Section	Mean Initial Rating	Mean Final Rating	Change	t*
A	2.20	2.00	−0.20	−0.939 n.s.
B	5.20	4.25	−0.95	−1.684 n.s.
C	9.45	9.55	0.10	0.998 n.s.
Total	16.85	15.80	−1.05	−0.846 n.s.

* A negative t value indicates a decrease in rating.

TABLE 10

Change in Rating (First Placebo to Final) and Its Significance for Non-Relapsed Group (20)

Index Section	Mean First Placebo Rating	Mean Final Rating	Change	t*
A	2.35	2.00	−0.35	−1.057 n.s.
B	5.10	4.25	−0.85	−1.276 n.s.
C	9.75	9.55	−0.20	−0.252 n.s.
Total	17.20	15.80	−1.40	−1.093 n.s.

* A negative t value indicates a decrease in rating.

TABLE 11

Relation Between Initial Rating and Per Cent Relapsing

Initial Rating	Number of Patients	% Relapsing
0–14	44	63.6
15–29	62	69.4
30 and above	38	78.9
Total	144	70.1

TABLE 12
Factor Analysis of 23 Items

Item	Symptom	Principal Axis Loadings (Decimal Points Omitted)						
		Factor						
		1	2	3	4	5	6	7
1	Hallucinations, attitude toward	75	−18	−40	−03	01	−31	0
2	Hallucinations, frequency of	79	−18	−38	−04	−00	−28	0
3	Delusions, plausibility of	42	03	−06	−44	29	11	0
6	Orientation (time, place)	26	−24	42	−08	−20	−28	2
10	Speech, coherence and relevance	50	−11	14	−26	−24	−30	2
11	Speech, decreased productivity	47	−41	07	45	−21	08	−2
14	Depression	27	71	−02	17	03	−09	2
16	Irrational rage reaction	28	−01	−45	47	06	33	1
20	Attitude toward hospitalization	11	−18	30	−31	−27	49	2
21	Affect	51	−39	33	11	−09	11	−0
26	Self-criticism	14	61	17	47	−03	−10	−0
29	Hostile attitude	32	06	−32	18	−22	49	1
30	Troubled by feelings of guilt	15	73	18	28	−12	−19	−0
41	Somatic complaints, other (specify)	31	18	−18	−43	02	05	4
42	Concern over sexual problems	38	73	04	−26	−24	09	−2
43	Frequency of concern over sexual problems	35	70	03	−29	−26	12	−2
46	Overeating	26	12	−11	−28	47	03	−3
48	Sleeping habits	34	07	36	01	44	40	−0
49	Drinking habits	−21	12	−39	06	42	−03	1
50	Smoking habits	09	00	38	−12	43	−08	−1
51	Dress	60	−34	29	22	26	−24	0
55	Mood, depressed	18	43	01	22	27	−02	4
62	Capacity for employment	52	03	46	18	14	23	0

will be interpreted, as only these contain correlations above .50. The first factor appears to be primarily related to schizophrenia, as the highest loadings occur with items involving hallucinations and delusions (1, 2 and 3), incoherent and irrelevant speech (10), affect (21), dress (51), and capacity for employment (62). The second factor seems to relate to depression, as the highest ratings occur with items describing this general syndrome: depression (14), self-criticism (26), feelings of guilt (30), and concern over sexual problems (42 and 43).

DISCUSSION AND CONCLUSIONS

The results clearly demonstrate that the index was successful in indicating relapses in an outpatient population. One might object that a bias was introduced by having the same examiner who diagnosed relapse make the ratings on Sections A and B. However, the ratings on Section C, done independently by a social worker, yield parallel results. A more nearly complete evaluation of the index, using ratings by independent raters not directly concerned with the management of patients, seems indicated. The results obtained, however, would apparently justify the use of the index for further investigation. The potential user might want to evaluate data from the three sections (A, B, C) separately; for this reason we have presented our data *in toto*. Since the three sections showed generally similar increases, the present authors found it more convenient to use only the total index scores.

SUMMARY

Details of the construction and validation of an outpatient rating scale have been presented. Investigation completed thus far indicates that the scale is reliable, and that

is valid when compared with clinical judgment of relapse.

In addition, a factor analysis was performed which yielded two clear factors in patients in remission: a schizophrenic factor and a depressive factor.

Because of the lack of simple evaluative instruments designed for outpatient populations, this scale should fill a definite need. Rating is rapid, scoring simple, and the scale seems to reflect the most important manifest changes observed in this group of patients.

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AN INTENSIVE FIVE-YEAR FOLLOW-UP STUDY OF TREATED PSYCHIATRIC OUTPATIENTS[1]

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INTRODUCTION

Follow-up studies of psychiatric patients have been concerned primarily with evaluation of the efficacy of various forms of therapy, and collection of data on the long-term course of psychiatric illness. Both of these topics have proved to be extraordinarily refractory, largely because we still cannot adequately describe patients, treatments or types of improvement, and because patients undergo so many experiences outside of therapy and subsequent to it that may affect them for better or worse. Hastings rightly says, "A review of the literature of prognosis in psychiatric illness is a lesson in humility, for it is difficult to obtain a clear picture either of spontaneous remission rates or how the rates differ from spontaneous when some treatment is used" (9, p. 1057). As will be seen, even this seemingly simple statement incorporates a knotty problem by using the adjective "spontaneous." Goldstein's recent paper criticizing the widespread use of the word "spontaneous" (7) lends support to the search for evidence which may clarify what Frank and his co-workers (3) have noted concerning patient-response to nonspecific activity of culturally defined healers. They have found evidence that reduction of symptoms is partially a function of the potentiation and activation of the patient's expectancies regarding such improvement.

This paper deals with long-term improvement, patient experiences subsequent to a treatment program, and relationships between these two factors. It is the report of a five-year follow-up study of a group of adult psychiatric patients who had originally received approximately six months of psychotherapy to which they were assigned in accordance with requirements of an ongoing research study. Subsequent treatment was uncontrolled but was ascertained at periodic follow-up evaluations. Results are considered from the standpoints of the long-term course of psychiatric illness and the effects of psychotherapy.

PROCEDURE

The patients were 54 white outpatients of the Phipps Clinic of The Johns Hopkins Hospital, diagnosed as having a psychoneurosis or personality disorder. Their ages ranged from 18 to 55 years, and about two-thirds were women.

Three forms of treatment were used: individual therapy, in which the patient was seen privately for one hour once a week; group therapy, in which groups of five to seven patients were seen for one and one-half hours once a week; and minimal contact therapy,[3] in which they were seen individually for not more than one-half hour once every two weeks. The first two therapies differed from the third in the amount of treatment time available to the patients, and differed from each other in the use of individual or group sessions.

Patients were assigned sequentially to one of the three types of treatment. Three second-year psychiatric residents each conducted the three types of treatment, with six patients assigned to each type. These therapists had nothing to do with the as-

[1] This study was supported in part by grants from the U. S. Public Health Service and the Ford Foundation.

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[3] Minimal contact therapy as described in this paper is labeled Continued Treatment Clinic (CTC) at the Outpatient Department of the Henry Phipps Psychiatric Clinic.

s gnment of patients or with evaluation of p tient progress. The design called for each p tient to receive six months of treatment, a d, although this goal was not quite a hieved, it was reasonably approached. Of t e 54 patients included in the study, 48 (89 per cent) had four or more months of t eatment during the experimental phase.

Patients were evaluated at the end of the six-month experimental period of treatment (or when treatment was terminated, if this occurred prematurely). Subsequently, an attempt was made to retain contact with the total group of 54 patients for four more re-evaluations, which took place six months after termination of treatment, one year following that, a year later, and two years later—that is, five years after the first treatment contact. Thirty patients returned for the five-year evaluation, and they comprise the sample presented in this paper.

Changes in personal discomfort and social ineffectiveness were selected as a means of gauging therapeutic efficacy (4). Change in discomfort was measured by a scale on which the patient indicated which symptoms he had and how much they distressed him. Measurement of change in social ineffectiveness was more difficult, since adequacy in interpersonal functioning cannot be determined simply by a person's behavior. The meanings of his actions to himself and others must also be taken into account before socially pertinent judgments can be made.

The solution was to devise a social ineffectiveness scale, consisting of 15 social interactional categories, such as over-dependent, over-independent, officious, withdrawn, impulsive, cautious and so on.[4] The patient was rated on each category on the basis of two observed interviews, one with him and one with a relative. Interviewer and observer in each case met following the session, resolved differences by consensus, and produced a combined rating. Finally, the four staff members (two for the patient and two for the relative) met for a staffing, during which differences again were resolved by consensus and a final rating was established for the patient. Though rather cumbersome, this procedure was considered to be necessary to minimize bias.

Throughout the five-year period the amount of formal help patients continued to receive was impressive. This treatment was obtained at the Phipps Clinic and elsewhere, either in parallel with clinic treatment or independently. Stimulated by the thinking of Hinkle *et al.* (10) concerning the possible role of life stress in the production of all types of illness, we gathered data which appeared to be pertinent to this question at each follow-up period. Events such as births, deaths, job changes, catastrophic events, illness of self or close friends and relatives, and seasonal fluctuations in symptoms and ineffectiveness were recorded. However, patient status could not be related directly to these intercurrent events, partly because of our difficulty in evaluating the *meanings* of these experiences to the patient, and partly because of the problem of disentangling changes produced by therapy from those produced by environmental events. For example, advancement on the job could be the *result* of improvement in behavior produced by therapy, or the *cause* of such improvement. The familiarity of these methodologic problems has not made them easier to solve. It was decided finally to

[4] The following are examples of definitions of social ineffectiveness scale categories:

2) *Superficially-sociable:* Seeks many superficial acquaintanceships. Breaks off relationships before they become intimate and/or refuses to form close relationships. N.B. Does not apply to relationships forced on patient, *e.g.*, family or necessities of occupation.

6) *Hyper-reactive:* Tends to over-react emotionally; behaves in an uncontrolled manner.

10) *Intra-punitive:* Self-critical; tends to blame, criticize and hold himself responsible for his difficulties, frustrations and failures. Over-apologetic.

Copies of this scale are available on request.

limit the analyses to intercurrent illnesses and treatment.

At each follow-up evaluation the interviewer completed a questionnaire with the patient. This was supplemented by another staff member's administration of the same questionnaire to a relative of the patient to record the relative's observations of these events in the patient's life for the same evaluation period. Information obtained included illnesses or treatments the patient had had during the follow-up period. These data were later categorized as follows:

1) Type of illness (medical, psychologic, or psychologically based somatic complaints).
2) Duration of illness (chronic or acute).
3) Severity of illness (major or minor).
4) Treatment resource (medical excluding psychiatric, psychologic including psychiatric, social work or religious counseling; or a medical resource for psychologic help).
5) Type of medication (for medical complaints or for psychologic complaints, *e.g.*, tranquilizers).

A special category under each heading was reserved for illnesses and treatments not clearly classifiable into one of the categories.

The data obtained were classified into the above categories by two independent judges, and differences between them were later resolved by consensus. It is acknowledged that the data are imprecise, in part because of the inadequacy of information on which the ratings were based, and in part because certain categories are not mutually exclusive. As an example, the same illness could be classified under two categories if the patient reported both psychic and somatic symptoms. On the other hand, each category was checked only once at any one evaluation point, regardless of how many different illnesses in that category the patient reported. Similarly, only one tally was made for a contact with a given category of treatment resource in a single evaluation period, no matter how many visits were made.[5]

The variation in the length of intervals between evaluations may also have been a source of inaccuracy, though the duration of the interval did not seem to be related to the *amount* of illness reported. The patient reports seemed to be based on a "psychologic present" which was relatively unaffected by the actual interval between evaluations. For this reason, no corrective factor was applied for intervals of differing time periods. Even in the face of these inadequacies, certain trends were apparent and will be presented in the section to follow.

RESULTS

The 30 patients who returned for the five-year evaluation did not differ in any important dimension from the original total group of 54 patients. That is, the initial test scores, diagnoses, types of treatment received and certain demographic characteristics of the five-year returnees seemed to be representative of the total original sample, which, in turn, was similarly representative of the general population of psychoneurotic white outpatients seen at the Phipps Clinic.

For the purposes of studying improvement and its correlates, the discomfort and social ineffectiveness data were analyzed independently. This was mainly because of an earlier finding (13)—that improvement is not a unitary phenomenon—and also because of our intrinsic interest in the two criteria and their components. There is some evidence that they might have been combined, with most improved and least improved patients categorized on the basis of ordering averaged ranks, as was done to study intercurrent illness and treatment. (See below.) For example, actual status (as opposed to change status) on one measure

[5] The records did not yield information as to the number of visits to a given treatment resource in any one evaluation period. If a patient visited the same resource in more than one evaluation period, the visit was tallied once in each period

was positively correlated with actual status on the other at the five-year point ($r = .61$, $p < .001$). There was also some association between *change* on each of the two measures at the fifth re-evaluation ($r = .38$, $p < .05$). However, if the measures had been combined to study improvement, this would have limited our opportunity to examine the subscales of the discomfort scale, as well as the different categories of the social ineffectiveness scale.

DISCOMFORT IMPROVEMENT

With respect to discomfort, at the five-year point 23 patients (77 per cent of the 30 returnees) showed improvement from their initial level, two were unchanged, and five (17 per cent) were worse. At the time of the first re-evaluation, immediately following the six-month experimental treatment program, 19 (63 per cent) of these 30 patients were improved and the remaining 11 (37 per cent) were worse. Thus, at the five-year point, the gains reported immediately following the formal treatment program had been maintained or increased.[6]

The mean status of the 30 patients as a group, at each of the evaluation points, supported the above finding. Figure 1 indicates that the group showed its greatest improvement between successive evaluations at the time of the first re-evaluation, immediately following the experimental treatment period. The mean symptom drop of 8.3 was significant ($t = 2.52$, $p < .05$). Thereafter the symptom level was generally maintained or showed a continuing slight drop, except for the third re-evaluation, when a slight increase occurred. The mean differences of 16.2 and 8.0 from the initial to five-year, and the six-month to five-year points, respectively, were both significant ($p < .05$).

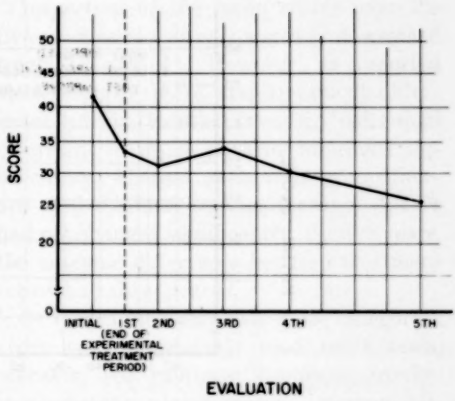

FIG. 1. Mean discomfort scale scores at each evaluation of follow-up returnees ($N = 30$).

Five subscales of the discomfort scale were examined separately. These consisted of statements to evaluate anxiety, and depressive, obsessive-compulsive, paranoid or somatic symptoms. Figure 2 demonstrates

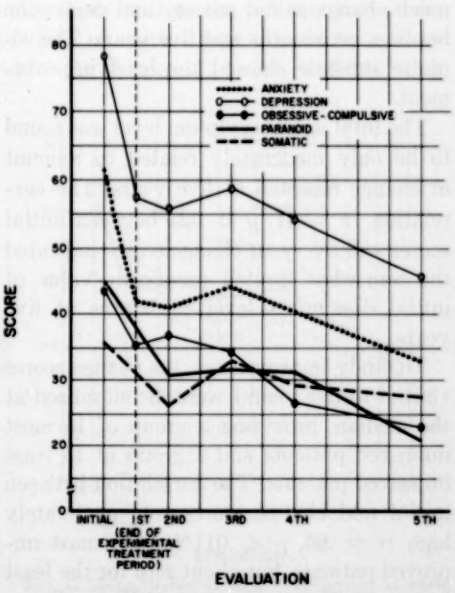

FIG. 2. Mean discomfort subscale scores at each evaluation of follow-up returnees ($N = 30$).

[6] Of the 16 patients who failed to return after the third evaluation, 12 were in the "most improved" category of the total sample, which is consistent with an earlier finding on the same population—that patients who showed most improvement at each evaluation were less likely to return for subsequent ones. This suggests that the improvement over time of a group of psychoneurotic patients is probably even greater than the figures in the text indicate.

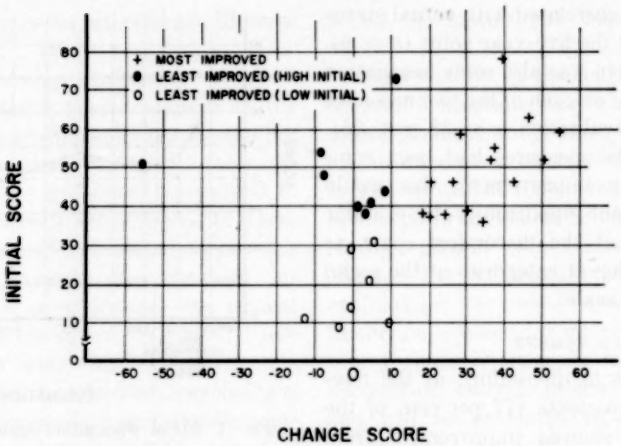

FIG. 3. Scattergram of discomfort scale scores: initial and change (to five years) of follow-up returnees ($N = 30$).

that all of the subscales showed significant improvement over five years (*ps* for all differences $< .05$). The anxiety and depression subscales showed rapid initial improvement and were the only ones to show significant change at six months ($p < .02$). The obsessive-compulsive and the paranoid subscales changed more slowly, but showed about as much change as did anxiety and depression between six months and five years. The somatic subscale showed the least improvement.

The total initial symptom level was found to be only moderately related to amount of change reported at five years. The correlation ($r = .41$, $p < .02$) between initial scores and five-year change scores indicated the somewhat limited prognostic value of initial discomfort level for status at five years.

To study improvement, the change scores (initial to five years) were dichotomized at the median, providing a group of 15 most improved patients and a group of 15 least improved patients. The correlation between initial and change scores was moderately high ($r = .66$, $p < .01$) for the most improved patients, but about zero for the least improved. The scattergram in Figure 3 presents these data. With respect to initial scores, the most improved were relatively homogeneous, whereas the least improved fell into two discrete groups, one of which had high initial scores and the other low initial scores. This finding is discussed below.

Although demographic (age, sex, etc.) and treatment (length, type) variables did not differentiate the dichotomized groups, certain other differences were apparent. The most improved had significantly[7] higher mean initial discomfort levels (49.6 to 33.9). They also presented more psychologic complaints on the average than did the least improved (37.7 to 21.8), but about the same average number of somatic complaints (11.9 to 12.1). In other words, the proportion of the total discomfort score which resulted from somatic complaints was much higher for the least improved than for the most improved. With respect to change at five years, all types of symptoms (*e.g.*, anxiety, depression) were significantly reduced in the most improved, whereas none was in the least improved. In fact, there was a tendency ($p < .20$) for the low improvers to have slightly greater scores at five years

[7] All differences (unless otherwise specified) were significant at less than the .05 level using appropriate statistical tests.

in he depression subscale, perhaps reflecting despondency over the relative lack of improvement. Two further findings were the lower mean age of the most improved (28.5 years to 33.7 years), and the tendency ($p < .20$ for the most improved to include more persons of the Catholic and Lutheran religious groups. Two-thirds of the most improved were Catholic or Lutheran, but only one-third of the least improved were. Another finding related religion and type of complaint. Nearly all patients whose initial complaints were exclusively or predominantly psychologic were Catholic or Lutheran, whereas the reverse was true for patients whose complaints were heavily weighted with somatic symptoms. Among the ten patients ranked highest on a psychic/somatic index, nine were Catholic or Lutheran; among the ten ranked lowest, only one was.

As noted above, the most improved were relatively homogeneous in terms of initial discomfort, which was high, as opposed to least improved, whose initial discomfort was either high or low. Patients who had not improved or were worse, who also had high initial scores, had a very high proportion of somatic symptoms. Incidentally, only two categories contained in the scale used to evaluate social ineffectiveness distinguished most- from least-improved initially. The most improved tended to be rated initially as less officious in their dealings with other people, and more intrapunitive.

INEFFECTIVENESS IMPROVEMENT

By the five-year evaluation, 29 (97 per cent) of the 30 returnees had improved in social ineffectiveness; one was worse. This surpasses improvement in ineffectiveness immediately following the six-month experimental treatment period, when only 20 (67 per cent) were improved, nine (30 per cent) were worse, and one was unchanged.[8]

[8] The relation between improvement and failure to return was similar to that found with discomfort (*cf.* footnote 6).

Inspection of the mean scores over the five-year period reveals a steady downward trend (Figure 4). However, as reported elsewhere (3), there were treatment differences at six months favoring individual treatment in which patients improved significantly, whereas patients in group therapy and minimal contact therapy (CTC) had not changed significantly. By five years, the outcome differences between treatment forms had disappeared.

Of the 15 ineffectiveness-scale categories, eight were infrequently used (withdrawn, officious, over-cautious, impulsive, overly-independent, unsystematic, irresponsible, and superficially-sociable), on each of which at least half of the sample had initial scores of zero, and less than one-quarter had scores of three or more (on a five-point scale). On four of the more frequently used categories (sexually maladjusted, constrained, intra-punitive and hyper-reactive), at least 60 per cent of the group received scores of three or more initially. On the remaining three categories (overly-systematic, extra-punitive and over-dependent), about half of the patients had scores of three or more. These distributions illustrate the kinds of social ineffectiveness

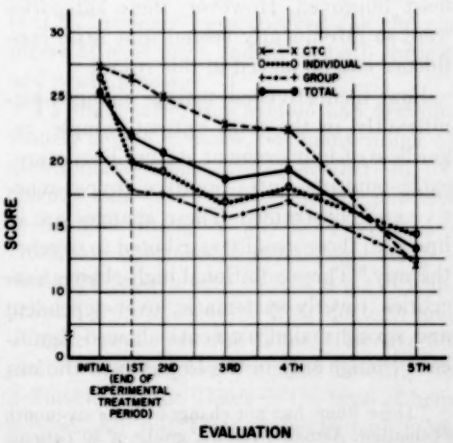

FIG. 4. Mean ineffectiveness scale scores at each evaluation by type of treatment of follow-up returnees ($N = 30$).

characteristic of a group of neurotic outpatients that contained no severely ill individuals.

As with discomfort, the ineffectiveness change scores were dichotomized at the median for the purpose of studying improvement. Of the demographic factors, only religion, where there was a tendency ($.20 > p > .10$) for more Catholics and Lutherans to be among the most improved patients, distinguished the two groups. With respect to initial test scores, the only finding was the tendency ($.20 > p > .10$) for least improved patients to have a higher proportion of somatic symptoms (as was the case with the least improved in discomfort). Those patients who had changed the most in social ineffectiveness at five years were initially more ineffective than were those who changed the least.

Degree of sexual maladjustment clearly differentiated the two improvement groups initially. Those patients who were most improved had higher scores (least well adjusted) in this category. Three other social ineffectiveness categories were predictive. Irresponsible and over-cautious were more characteristic of the most improved patients, as was superficially sociable of the least improved. However, these categories were so infrequently scored that little confidence can be placed in this result.

Four ineffectiveness ratings changed significantly in the total patient sample, regardless of improvement status. They were: extra-punitive, intra-punitive, hyper-reactive and constrained. These changes are in line with those usually attributed to psychotherapy.[9] Three additional high-change categories (overly-systematic, over-dependent and sexual maladjustment) showed significant change only in the 15 patients who improved most.[10] This means that the difference between most improved and least improved in ineffectiveness was attributed chiefly to these.

INTERCURRENT ILLNESS AND TREATMENT

The following results are based on information gathered beginning with the second re-evaluation, six months after the completion of the experimental treatment period, since primary interest was in illnesses and treatments subsequent to that period. The total time span over which the reported data were gathered is four and one-half years. Ideally, 120 evaluations should have been recorded for the 30 patients during this time; however, not every patient kept his scheduled appointment. A total of ten evaluations were missed at various points among the 30 patients over the study period, leaving a total of 110 evaluations available.

To consider first the amount of illness as determined by patients' complaints, of the total of 110 *evaluations*, no illnesses at all were reported in only 12 (11 per cent). However, every patient reported at least one illness in the four and one-half year period.

With respect to type of complaint, medical complaints were many times more frequent than were psychologic. Of 142 classifiable entries of illnesses, only about one-third were clearly psychologic, the rest being about equally divided between strictly medical, and medical with a psychologic component. Results were similar when tabulations were made per patient. Twenty-three patients reported psychologic illnesses at least once, 26 reported medical illness, and 21 reported medical complaints which had a probable psychologic involvement. Since there were only 30 patients, it is apparent that illness was frequently tabulated under more than one category. Twenty-

[9] These items had not changed at the six-month evaluation. Actually, for the group of 30 patients there were no significant item changes at six months.

[10] Of these items, only sexual maladjustment had changed at six months in this group of 15 patients.

seven patients were scored in more than one category, and 13 of these were scored in at least three. Only three patients were checked in just one category.

The patients sought help quite frequently. A visit to a helper was checked at least once in 92 (84 per cent) of the 110 evaluations. Twenty-seven patients (90 per cent) visited a helper in all or in all but one of the evaluation intervals, and every patient made at least one such visit in the four and one-half years.

The type of help sought and type of medication received reflect the type of illnesses reported. Of 103 classifiable entries, one-third were contacts with a psychologic helper (including psychiatrists); the remainder were with non-psychiatric physicians,[11] half for relief of emotionally based symptoms. All but three patients went at least once to a general practitioner, and nine went *only* to this type of physician, but almost two-thirds of the 30 patients got help for emotionally based symptoms from these physicians. Of 21 patients who sought help from a counselor or psychiatrist at least once during the four and one-half years, 19 also visited a general practitioner at least once. Two-thirds of the total sample went to more than one helper in an evaluation period at least once during the total time span, and five reported these multiple visits on three or more evaluations.

In terms of medication received, of 59 classifiable entries, 41 were sedatives or stimulants, and 18 were medications for specific medical illnesses. Most of the 32 unclassifiable entries represented aspirin or vitamins, which in most cases probably could have been included in the psychologic category. Twenty patients took sedatives or stimulants at least once, and ten took other medications. Sixteen patients took more than one type of medication, and 16 reported taking medicines on all or on all but one evaluation, whereas ten received medication during no more than one evaluation period.

With respect to severity, illnesses were classified as "major" if they caused a severe impairment of the patient's usual functioning, including illnesses requiring hospitalization; as "minor" if this was obvious from the notation (*e.g.*, "colds"), or if there were no visits to a physician, or little or no medication taken. On this basis, there were 13 major illnesses among ten patients, all of whom also had minor or not clearly definable illnesses. At the other extreme, six patients had minor illnesses only. The remaining 14 had at least one illness of indeterminable severity, and all but one of these also had at least one minor illness.

With respect to duration, the number of chronic illnesses was probably over-represented in our tallies, since, if a chronic illness was reported at more than one evaluation, it was checked each time, although it might have represented a single continuing illness. On the other hand, no matter how many acute illnesses a patient reported for one evaluation period, the category was scored but once. It is of interest, however, that 27 patients were checked for chronic illness at least once, and 23 for both chronic and acute illnesses.

To summarize, this sample had few obviously severe illnesses, but many minor or questionable ones which tended to be chronic and were regarded by the patients as medical, though a physician would see most as having major psychologic components. Patients tended to seek a lot of help, primarily from local medical doctors, and to take medicines for relief. The overall picture also suggests frequent inability to define clearly the illness or the type of help needed. All but three patients had more than one type of illness, and most visited general medical practitioners as the pri-

[11] For ease of exposition, these will be referred to hereafter as general practitioners, although actually a few medical or surgical specialists are included.

mary source of help at least once. However, the most common reason for doing this seemed to be for relief of psychologically based complaints. In the same connection, about two-thirds visited more than one treatment source in a given evaluation period, and about one-half took more than one kind of medication.

To investigate the relationship between improvement and intercurrent medical history, the patients were dichotomized on the basis of their combined rank orders of improvement in discomfort and ineffectiveness.[12] The least improved reported more illnesses (88 to 66) and more treatment contacts (83 to 55) than did the most improved. Of the 15 patients who visited a help-giver in *each* evaluation period, ten were in the least improved group; of the 15 who did not see such a person during at least one interval, only five were in this group. (These findings can be seen as an independent validation of the improvement measures—that is, patients who were sicker on the criterion measures had more illnesses and treatments than did those who were less sick.)

With respect to clarity of definition of illnesses noted above, some especially interesting differences appeared in the nature of the complaints reported by the least and most improved, and in the pattern of help-seeking. Clearly psychologic and clearly medical illnesses were noted with about equal frequency in the two groups. However, the greater number of illnesses among least improved patients was accounted for almost completely by illnesses perceived by the patients as bodily but by the raters as having a psychologic basis, or by illnesses which the raters could not classify clearly from the patients' reports of their complaints. The least improved had 24 entries of medical complaints on a psychologic basis; the most improved, 16. The least improved had 11 entries of unclassifiable illness among six patients; the most improved, only one. Also, all five patients who had two or more notations of this type of illness were among the least improved.

Consistent with these findings, the least improved made contact with general medical practitioners over twice as often as did the most improved (47 to 20). The preponderance of contacts for the least improved was for illnesses they apparently regarded as medical but the raters regarded as psychologic or unclassifiable. This tends to explain why they went to general medical practitioners more often for help with emotional problems than did the most improved. These findings suggest that the least improved were inclined to define their complaints in such a way that they could not get adequate help, either because they went to the wrong resource or because the complaints were so poorly expressed. Confirmation of this assumption is suggested by the finding that the least improved did more "shopping around" than did the most improved. All five patients who reported using every type of treatment resource (medical, psychiatric, non-medical, psychologic) during the four and one-half year period were among the least improved.[13]

To check this impression further, a special tally was made when the record indicated that a patient had had more than one treatment contact in a given evaluation period. Over the four re-evaluations, this was noted 21 times for the least improved and 14 times for the most improved. Of the ten patients who were in contact with more than one help resource within more than one of the four evaluation periods, seven were among the least improved.[14]

[12] The two scales were analyzed separately, producing essentially the same results. For reasons of economy, only the results of the combined criteria are presented.

[13] In the same connection, all three contacts with chiropractors were made by members of the "least improved" group.

[14] When the 45 patients who returned for the third evaluation (at two years) were dichotomized into most and least improved, the relationship between ambiguity and failure to improve did not appear; nor was it noted in the five-year sample at the two-year evaluation. Apparently this tendency emerges only over a long period of time.

The relationship between difficulty in obtaining appropriate help and lack of improvement was found also with respect to resources for psychologic help. Consistent with the equal frequencies of illnesses classed as clearly psychologic for both groups, the most and least improved were also noted almost equally often as seeking help from a psychologic treatment resource (23 to 26). On the other hand, if these resources are grouped as "psychiatrists" and "other,"[15] the most improved, in contrast to the least improved, went to psychiatrists much more often (18 to five, respectively). Again it appears that, even when the least improved reported their complaints as clearly psychologic, they were unsure about, or unaware of, the psychiatric resources available in the community.

DISCUSSION

In any attempt to interpret these findings, it must be kept in mind that the analysis has disregarded individual differences among patients. In all probability, certain patients are concealed in the statistical results who responded unusually well or poorly to treatment. A detailed study of them would undoubtedly provide important information concerning psychotherapy which investigations of this type cannot yield. Furthermore, the initial period of therapy was very brief, and was conducted by relatively inexperienced psychiatrists. It is possible that longer treatment by more mature practitioners might have yielded different results. Nevertheless, the consistency of many of our findings with those reported by others suggests that they possess some validity.

As a group, these run-of-the-mill adult psychiatric outpatients improved over the five-year observation period, despite the fact that they continued to report intercurrent illnesses. Improvement in discomfort occurred rather early, whereas improvement in social ineffectiveness, though more pronounced at five years, occurred less rapidly. Symptoms of anxiety and depression are characteristically relieved by expectation of help, and from previous studies it may be surmised that the improvement noted at six months actually occurred much more promptly (6). The apparently more gradual improvement in other types of symptoms suggests that other processes must be involved that probably also account for the continuing improvement in anxiety and depression after six months. What these processes are remains obscure. The least changed symptoms at five years were somatic ones, and these symptoms were especially characteristic of least improved patients throughout the study. This is in accord with general experience.

With respect to social effectiveness, the group as a whole became less hyper-reactive, less constrained, less intra-punitive and less extra-punitive. These changes may simply reflect general alleviation of subjective distress, enabling patients to achieve more spontaneity of emotional expression within a narrower range, and giving them less occasion to assign blame either to themselves or to others. This may also reflect greater acceptance of the desirability of objectivity—a value implicit in psychotherapy.

Diminution in overly-systematic behavior, over-dependency and sexual maladjustment was significant only in the patients who improved most. The first change is compatible with greater spontaneity; the second has also been reported by Lorr (12), and the third accords with general experience. It probably results in part from improvement in anxiety and depression (which tend to inhibit sexual interest and activity), and may also reflect a greater capacity to integrate intimate relationships.

For the group as a whole, the improvement at five years was so great as to obscure the differences in improvement related to different forms of psychotherapy noted immediately at the close of the experimental period. Similarly, Levitt (11) found that three-quarters of a population of treated

[15] Thirteen of the 20 contacts with non-psychiatric resources for psychologic help were with clergymen, the rest having been social workers and psychologists.

neurotic children reported improvement at a one- to ten-year follow-up, as compared to only two-thirds immediately at the close of of treatment, and that improvement was a function of time. Denker (2) found that 45 per cent of his sample of adults treated by insurance company physicians for emotionally based complaints were discharged as recovered at the end of one year, 91 per cent at the end of five years. Whitehorn (16) found that after five to ten years the improvement rates of schizophrenic patients of the less successful psychiatrists had overtaken those of the more successful ones.

Similar results have been found with physical treatments. Thus Scoville (15) reported that various series of psychiatric patients who had undergone orbital undercutting showed marked benefit in from six to 64 per cent immediately after treatment. Four to 11 years later the rate was appreciably increased in every series, the incidence of marked benefit ranging from 38 per cent to 100 per cent. Zubin's (17) statistical studies of hospitalized patients showed that, after five years, results were no better for those who had received treatment than for those who had "spontaneously" recovered. In view of the amount and variety of help obtained by patients in this study, it seems plausible that much of their long-term improvement may be attributable to non-specific treatment (7), offered chiefly by physicians.

It has been generally assumed that the evaluation of the effectiveness of different forms of psychotherapy depends ultimately upon follow-up studies. The findings of this study raise serious questions as to the validity of this premise. The cumulative effect of the life experiences of psychiatric patients after the termination of treatment, coupled with the impossibility of determining what part of these experiences should be attributed to changes in the patients' behavior resulting from therapy, would make it wellnigh impossible to determine what features of the end state can be attributed to a course of treatment years before (1). The problem is identical with that of evaluating different methods of child-rearing in terms of the finished adult products.

A more nearly valid evaluation of different forms of psychotherapy might be in terms of their ability to accelerate improvement. Even if the results of all forms of therapy prove to be statistically indistinguishable after a sufficient lapse of time, the better therapy would be the one which achieved the greatest amount of improvement most rapidly. A prompt and sizable reduction in the duration of a patient's suffering and disability would be sufficient justification for psychotherapy. If this view is correct, evaluation of different forms of psychotherapy should be primarily in terms of their immediate results.

That younger patients improve more than older ones is compatible with general experience, though this relationship did not appear with respect to improvement in social ineffectiveness. The relationship of symptomatic improvement to religious affiliation, however, was quite unexpected, and we would be inclined to dismiss it as a chance result were it not that it also appears to be related to responsiveness to placebo.[16] A possible explanation is that Catholics and Lutherans, as members of relatively authoritarian denominations, may be more prone to respond favorably to the prestige and authority of physicians, especially as agents of an institution with status in the community. Why they have a greater proportion of psychic complaints than the remainder of the sample, however, is obscure.

This paper has presented certain features of the intercurrent illnesses and treatments, and their relation to improvement. In the absence of a control group we cannot say how the health record of these patients compares to that of the population at large. The variety of illnesses reported, however, supports the finding of Hinkle *et al.* (10), that

[16] Unpublished data from studies now in progress by the authors.

stress increases the incidence of all types of illness.

In accord with general experience, it was found that patients who express their psychologic problems in bodily terms and seek medical treatment for them do less well than do those who recognize the psychic causation of their difficulties. It will be recalled that the least improved patients compared to the most improved had a much larger percentage of illnesses classified by the raters as bodily complaints on a psychologic basis. The least improved also much more often reported their complaints in ambiguous terms than did the most improved, went to general medical practitioners for them, and visited more different kinds of treatment resources. Since these findings appeared only at the five-year re-evaluation, the extent to which they can be generalized is not certain. They do, however, appear to be related to certain findings reported by Freedman *et al.* (5) with ambulatory schizophrenics who were receiving placebos. In their work, they found that patients who denied mental illness were more likely to be driven out of treatment by physicians with an active, "warm" attitude than by those with a detached attitude. Those who accepted the fact that they were mentally ill remained in treatment with warm physicians but promptly terminated contact with detached ones. That is, whether a patient remained in treatment depended upon the consistency of his expectations with the type of relationship offered by the physician. Furthermore, an inconsistent doctor-patient relationship militated against a favorable response to placebo (8).

Taken in conjunction, these findings suggest that failure to improve is related to inadequate communication between sufferer and helper. There may be several reasons for this. If patient and doctor cannot clearly define what is wrong, this in itself creates anxiety. If the patient goes to a general physician, he fails to gain relief for psychologically based symptoms because the treatment is inappropriate. If a patient who somatizes his problems goes to a psychologic resource he fails to get help because psychotherapy operates by words; such patients typically have difficulty verbalizing their problems, and these cannot be readily modified by the words of others (14). Furthermore, to the extent that psychotherapy is a form of problem-solving, unless patient and therapist can agree on the nature of the problem little progress can be made. The patient's inability to express himself clearly obviously impedes reaching such an agreement. Failure to gain relief causes the patient to go from one resource to another, which aggravates his distress by heightening his conviction that no one really understands what to do for him.

Thus ability to profit from treatment seems to be related to the clarity of the patient's perception and communication of his symptoms, and the ability of patient and therapist to agree on their nature and treatment.

SUMMARY

In this five-year follow-up study of adult psychiatric outpatients, the focus has been on the long-term course of psychiatric illness, as well as on the effects of psychotherapy. To evaluate the long-term course, special attention was given to intercurrent illnesses and treatments. The major findings were as follows:

Improvement in discomfort immediately following treatment was primarily in anxiety and depressive symptoms. By five years, all types of symptoms were substantially reduced.

Patients found to be most improved in discomfort at five years were younger, more likely to be Catholic or Lutheran, and more likely to have been sicker initially on the criterion measure. Those least improved had many somatic symptoms, both initially and at five years.

Improvement in social ineffectiveness, though less than discomfort change, was

marked at five years. Differential effects of treatment forms found immediately after the initial six-month treatment period had disappeared by five years.

Patients most improved in social ineffectiveness seemed to have gained greater spontaneity in their intimate relationships, and this was associated with greater personal comfort.

As a group, though they steadily improved, patients reported a great deal of intercurrent illness and treatment throughout the follow-up period. Much of the illness was reported as medical and was treated by a general medical practitioner, but often with sedatives and stimulants.

At five years the least improved patients were more likely than the most improved to define their illnesses in bodily terms, or to expresss them ambiguously. Consistent with this, they sought more different kinds of treatment resources, which, in turn, may have aggravated their condition by increasing their confusion. Thus, ability to respond to treatment may be a function of the patient's success in communicating his difficulties to the therapist, and the acceptance by patient and therapist of a course of action.

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SCHIZOPHRENIA AND THE MARITAL FAMILY: IDENTIFICATION CRISES[1]

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We have been studying a group of 17 women who, as young adults, had experienced massive and repeated difficulties in their marital families, and had eventually been hospitalized as schizophrenic. We have suggested elsewhere (15) that these women foundered upon two broad tasks presented by participation in the marital family: detaching themselves from the claims of parental ties, and synthesizing conflicting childhood identifications within a workable adult identity. We have further proposed that the crises they experienced in assuming the responsibilities and prerogatives of marital family life may illuminate, and in turn be illuminated by, three inter-related lines of inquiry (17). The first concerns the vicissitudes of developmental stages in early childhood which form the inner basis for mastering the progressive demands of adult life. The second concerns the nature of the developmental tasks posed within the contemporary family, those "requisitions in reality" (6) upon which these women foundered. Finally, we may turn to contemporary family patterns which provide the concrete environmental context within which the conflicts of these young women are modified, perpetuated or exacerbated.

The present report applies these interrelated perspectives to a single case in order to trace in some detail the way in which these factors complement each other in shaping the vicissitudes of a developmental transition. The young woman, whom we shall call Mrs. White, experienced in marital life a severe crisis of identification resulting in a psychotic episode. We shall examine her crisis from each of the three perspectives in turn, demonstrating how each contributes distinctively to our understanding, and how all are interwoven in the single but complex reality of her episode. Finally, we shall consider in more general terms the implications of this analysis.

EARLY DEVELOPMENT AND THE CRISIS OF IDENTIFICATION

Kate White, the second of two daughters, had been a difficult pregnancy and birth for her mother, who was confined to bed for a month post partum. In the first year of life she had had two serious illnesses, necessitating much special care. These experiences, frequently recounted by her mother, contributed to a later sense of fragility and lack of appeal. Her sister's docile obedience contrasted with her own active rebelliousness; the sister was quite clearly the mother's favorite, the patient became the father's.

The father replaced the mother in Mrs. White's life at a very early date. His marriage was an unhappy one, and this daughter became the focus of his life within the family. He provided the love and affection Mrs. White felt she did not get from her mother, and she was pleased by her

[1] This report is based on a study carried out by the California Department of Mental Hygiene and partially supported by Grant 3M-9124 from the National Institute of Mental Health.

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pre-eminence in his life. This attachment became prepotent, and complemented and supported the turning from her mother.

Mrs. White's positive identifications were with her father rather than her mother, and her sexual orientation became significantly masculine. This development was reinforced by the unrewarding picture of female life portrayed by her mother. Although her father had been carrying on an affair with his secretary for many years, the parents had decided to remain together, ostensibly for the sake of the children. The affair was known to the mother, but initially was kept from the children (who, however, had reason to suspect it). When Mrs. White was eight or nine years old her mother openly confronted the two girls with their father's infidelity and made a direct bid for their sympathy and allegiance. Woman was portrayed as leading a precarious and painful existence, and as facing the constant threat of desertion and victimization, an estate with little appeal or opportunity for satisfaction.

The father's extramarital affair, combined with his already strong attachment toward his younger daughter, served to intensify the erotic implications of their relationship. During latency Mrs. White had become a tomboy who identified with her father's active, independent and rebellious attitude toward life. She took his professional career and his philosophy as models. At adolescence, however, when her relationship to boys became an issue, her father's jealous possessiveness posed a problem. Even when she had become a woman, her father intervened forcefully to disrupt a relationship she had excitedly and fearfully begun to form with an older married man. She was chagrined at her father's arbitrary and authoritarian intervention in this illicit relationship, but was also pleased by the demonstration of his possessive love and interest in her.

Mrs. White's attachment to her father thus provided a substitute for the maternal attachment, but reinforced her negative response to her mother and to womanhood, provided a masculine model for identification (complicated by infidelity), and established a gratifying erotic tie difficult to relinquish in favor of later potential love objects.

At the time of her psychotic episode, Mrs. White's marital family repeated many significant features of her parental family. (The relation of such cases to the carefully documented "anniversary reactions," identified by Hilgard and her co-workers (7–9), is discussed by the writers (15) elsewhere.) The marital family consisted of two daughters, a father away from home much of the time, and a mother who felt martyred. Some of Mrs. White's symptoms reflected a reliving of distressing aspects of her own childhood through identifications with her children. She felt her daughters had come into possession of some terrible information which they should not have had, and that this unspecified knowledge would cause them great harm. The children seemed to be giving her "signs" that indicated she should leave her husband. She decided she would not burden them with her troubles as she had been burdened by her mother, but worried about the effect on them of the precarious condition of her marriage. The children's faces seemed to her to have "lost their light."

Other symptoms mirrored aspects of her own erotic attachment to and identification with her father, mobilized in part by the circumstance that both of her own daughters were of Oedipal age. Thus she was preoccupied with guilt about an extramarital affair (to be discussed in the next section), with fantasies of having affairs, and with suspicions that her husband was unfaithful. Briefly before hospitalization she consulted a psychiatrist; when he replaced his secretary, Mrs. White thought he did this in order to have an affair with her, and came to believe she was married to him. Religious ideation and protestations about the "sanc-

tion of marriage" expressed a guilt-dominated identification with her mother. Recurrent ideas that her husband was a latent homosexual expressed her own confused sexual identity in projective form.

Thus, in the crisis preceding hospitalization, Mrs. White was confronted with various identification fragments (and defenses against them) which had previously remained in partial dissociation. These images of herself—as a confused little girl in possession of secret and terrible information; as a long-suffering, neglected and betrayed wife and mother; as a frustrated, masculine careerist; as an Oedipally victorious daughter, and as a promiscuous philanderer—were related to earlier identifications formed in relation to a frustrating and martyred mother, a prepossessing male mother-substitute, and a seductive, jealous, Oedipal father. (Two years subsequent to this hospitalization we learned that Mr. White had just begun an affair with his secretary. He planned to keep up the appearance of a marriage for the sake of his children. Thus, the earlier family drama was virtually replicated in reality in the marital family.)

ADULT DEVELOPMENTAL TASKS AND THE CRISIS OF IDENTIFICATION

At the time of hospitalization, Kate White was 36 and the mother of two girls, one five and one three and one-half. She had been married for 13 years, and had recently moved into a home of her own for the first time. Near midnight one evening she entered her church, took off her wedding ring and placed it on the altar. This bizarre repudiation of her marriage symbolically located the area of her current troubles within the marital family.

From the beginning, Mrs. White's marriage had been marked by inner reluctance and uncertainty. She had met the young man in college. He had the same career interests she and her father shared, and she was feeling increasingly restive at home (in part because of the continuing erotized attachment to her father). She had expected no consuming love in her life, and decided to marry because it seemed the appropriate thing to do; following college and a brief period of working, she unenthusiastically accepted the young man's proposal.

The Whites began what was to be a prolonged phase of rootlessness as Nelson White wandered from job to job and town to town trying to find himself. Mrs. White's role in the family was ambiguous: at times she was a competent career woman who worked to further her husband's education; at other times she deferred her career to his, and depended upon vicarious identification with his activities for a sense of accomplishment. Feminine domestic activities were difficult for her and provided little sense of satisfaction and pride. Her husband's preoccupation with his own career left her feeling abandoned, incompetent and unloved.

As we have noted, Mrs. White had been a tomboy—active, outspoken and rebellious, a replica of her father. Marriage now made demands for heterosexual intimacy and emotional involvement, and for a more feminine participation in family life. She found herself unable to enjoy or to perform in domestic roles to her own satisfaction. She could not feel a sense of communality as a woman with her friends and neighbors. She could not fully transfer attachment from her father to a new man in a new nuclear family, and felt increasingly alienated from her husband. Later, when she had children, she felt hopelessly tied down and frustrated in her career aspirations, and guilty about these feelings in relation to her daughters. She felt she would "go crazy" if confined to domestic routines. Elements of sexual confusion and dissatisfaction with her role as a woman were represented in the recurrent idea that her husband was a homosexual.

Concerns about marital infidelity also dominated her marriage. She often felt intense temptation and intense guilt over

fantasies of affairs. She sometimes suspected her husband of infidelity, and at one time felt "relieved" when her suspicions seemed to be confirmed: she had anticipated that this would happen, and was reassured that it did not "shatter" her.

Her one actual affair is worth considering in greater detail, as it played an important role in her preoccupations and in her prehospital crisis. The man, married and a father, was the one to whom she had been drawn before her marriage. Their incipient affair had been quashed by her father's intervention, but the man visited her some years later, shortly after her father's death, at a time when her marriage was in a phase of silent isolation. Her husband was working in a distant city on one of many transient jobs, and she was feeling lonely, unloved and unappealing. Her husband's indifference to her, and his outside interests, had contributed to her sexual confusion. The man pressed his suit; she briefly felt more desirable and appreciated, and engaged in a short affair.

Shortly afterward Mrs. White decided to have children. This decision was partially prompted by a desire to consolidate her marriage and to secure her sexual identity. She hoped that children would involve her husband with the family and provide her with a sense of commitment to her role as a wife. Instead, motherhood increased her distress, dissatisfaction, and sense of alienation.

Some years later, just prior to her breakdown, this man visited her again and proposed that they resume their affair. During the course of his visit she experienced the presence of her dead father in the house, and discontinued the relationship. The visit came at a time of growing anxiety and sexual confusion, as she faced "settling down" for the first time in a home of her own, as a wife and a mother, but with a husband who seemed uninvolved with her and to whom she could not reach out for support of her precarious female identity. The other man's continued interest in her as a sexual partner was both a reassurance and a danger; she sought hallucinatory gratification in the form of her father's return and protective concern for her.

Thus, marital life posed complex dilemmas for Mrs. White: she felt pulled in contradictory directions by a continued involvement with her father-imago in the face of demands for transfer of this love to her husband, as well as by the presence of masculine identifications and homosexual trends which stood in opposition to the requirements for female heterosexual participation. The theme of infidelity and its associated identifications was also revived. Marriage and motherhood imposed strategic and insurmountable developmental demands for relinquishment of parental attachments and for the synthesis of conflicting identification elements within a workable adult identity.

MARITAL FAMILY PATTERNS AND THE CRISIS OF IDENTIFICATION

Throughout most of her marriage Mrs. White had held to the view that her feelings of alienation and distress were the result of external uncertainties, in particular the rootless, almost nomadic life she and her husband were leading. She retained a hopeful fantasy of settling down to a secure place in a fixed community, of "sinking roots," and thereby coming to feel at one with herself and her commitments. As we have seen, her severe personality disorganization began shortly after the Whites bought their first home and experienced, if only transiently, a sense that after more than a decade of marriage they were really settling down.

It is evident that the extended period of rootlessness had deferred for Mrs. White the necessity of synthesizing within herself the incompatible identifications described earlier. It had also permitted her to deny the significance of her inner feelings, to avoid the terrible realization that she had always

fel detached from people, empty, inadequate, and sexually confused. It had facilitated the projection onto her husband of her own sexual confusion and homosexual tendencies, of inability to settle down and make firm commitments to adult roles and relationships, and of promiscuous fantasies.

The partial effectiveness of these defenses hinged upon marital family patterns which could reinforce and support them. Perhaps the simplest way of designating the conjoint pattern of accommodation evolved by this husband and wife is as a *state of suspension*. By this we mean that the marital family accommodation was characterized by an explicit moratorium on commitments to personal and social roles, by ambivalent explorations of multiple careers and living situations, and by interpretations to each other that experienced troubles were the results of being "unsettled," and would be terminated in a future time when roots would finally be established.

The White family was maintained for many years in this state of suspension, supported by a myth of "becoming." They indicated by unspoken mutual agreement that lasting commitments were not made, that valued self-images were still to be sought in the future, and that existing unsatisfactory identities were only transient. Both husband and wife were exquisitely complementary in their sense of impermanence, and in their continuing quest for adult identities. Mr. White's career line was marked by devious explorations and manifold uncertainties in the face of open horizons. He moved from good jobs in one field and locale to good jobs in other fields and locales, and retained vague fantasies of returning to school sometime to learn yet another profession. Mrs. White, for her part, experimented with a career, marriage and motherhood, constantly hoping for a sense of appropriate fit and meaningful commitment.

Mr. White gave small evidence of what he was really like or what his life plans were. He maintained a kind of distant ambiguity which provided little reality contradiction for his wife's projections onto him. His defensive preoccupation with nonfamily matters, his occasional vague hints of extramarital sexual interests, and his avoidance of a masculine role as effective head of the family furnished occasional reality support for her projections.

The style of marital life achieved by the Whites supported and maintained critical defensive structures for both partners. However, the consequences were double-edged. While suspension of commitments avoided certain dangers, it simultaneously perpetuated the uncomfortable sense of personal role diffusion and of social isolation. Mr. White's uncertainty in his career, in his commitment to his marital role and as a mate complemented Mrs. White's sexual confusion, her ambivalence about marriage and motherhood, and her sense of estrangement.

This form of marital accommodation could not survive without change the gradual aging of the couple beyond that youthful period when the future seems to lie open before one, and, most particularly, the advent of children. With the birth of children Mrs. White reluctantly subordinated her career aspirations to meet the demands of motherhood. This change required a commitment which neither husband nor wife was able to make, and emphasized for Mrs. White the conflicts inherent in her identification with her father and disidentification with her mother.

Thus, when the couple finally attempted to settle down (in part because their older daughter was nearing school age), former patterns of family accommodation became increasingly untenable. Mr. White consented to buy a house, and for a while threw himself enthusiastically into his new work. After a time, however, he began to talk seriously about attractive career possibilities at an overseas location, and to lose interest in the daily requirements of his

immediate job. Mrs. White, threatened by both the demand for commitment and the threat of impermanence, became more and more disturbed. We may judge that the family could not relinquish that state of suspension which had served to accommodate each partner's conflicts, and that the increasing necessity to do so served as one critical precipitant of what was at once a family crisis of commitment and a personal crisis of identification for Mrs. White.

DISCUSSION

This report on a single case bears on certain more general considerations to which our work had led us. Our study of young schizophrenic mothers and their marital families supports the viewpoint that significant personality development extends beyond the childhood years. We agree with Benedek (1, 2), Bibring (3) and Erikson (5) that there are developmental phases in adolescence, early adulthood and beyond, with characteristic crises and characteristic developmental tasks to be mastered. In our research, attention has been focused on the developmental problems of early adulthood, with marriage and parenthood looming as the most important personal and social transition points to be negotiated. The intrapersonal and interpersonal requirements for satisfactory participation in these later developmental phases have been highlighted through observations of gross failures to accomplish these progressive transitions.

Clinical observations and theoretical considerations alike led us to the conclusion that marriage and parenthood require both separation from competing parental attachments and the integration of critical ego identifications. For the wife, participation in marital family life necessitates the establishment of an appropriate female identity which will promote intimate heterosexual relations, feminine role performance, and child rearing. Motherhood further presses for the synthesis of positive female identifications—in particular, identifications with the young mother's own mother—as an important source of maternal drive.

At the point of personal and marital family breakdown we were impressed by the operation of a complementary series of internal and external events, by the co-ordination of reciprocal intrapersonal and interpersonal stresses acting in transaction to jeopardize individual and family function. Contemporaneous marital family demands were strategically linked to certain critical difficulties in earlier epigenetic development. In the case presented, the demands of marriage and parenthood pressed for resolution of persistent defects in identification. These identification defects were founded on early conflicts which were reawakened by progression into the marital and parenthood phases of life.

An added condition involved in the personal and family disorganization was a disruption of a pattern of family accommodation. We found that the contemporaneous struggle with identification crises in the marital family was shaped and molded by the conjoint participation of other family members. Accommodative patterns of family relations were evolved that complemented individual mechanisms of defense. Family structure, however, not only tended to support and maintain individual compromise solutions to conflict, but also functioned to perpetuate the precarious equilibrium, providing neither the opportunity for continued progressive development nor the possibility for simple regressive retreat. The simultaneous co-ordination of increased pressure, both internal and external, for the integration of crucial identifications, and the disruption of family patterns of accommodation, provided the occasion for personal and family disorganization. At this point the behavior of the young wives became unmanageable, and they were removed from the family to the local state hospital.

Implicit in these concepts is the idea that, to mature, the individual requires stepwise psychologic development, co-ordinated with changing patterns of social participation. The individual creates and is in turn created by his environment. To date, the main emphasis in the literature on the family of schizophrenics has been on the parental family (4, 10–12, 18). These studies have suggested the importance in the genesis of schizophrenia of family schism (13), parental psychopathology (12), the transmission of family irrationality (14), and the inability to accomplish differentiation and autonomy (16). Our studies call attention to another dimension, as we have focused on the importance of the marital family of young schizophrenic mothers, emphasizing the strategic demands made by adult family participation as it plays upon earlier developmental defects. Thus we call particular attention to conditions of precipitation, conditions which bear a complementary relationship to defects evolving out of parental family relationships and early personality development.

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MURDER OR EPILEPSY?[1]

A. EARL WALKER, M.D.[2]

INTRODUCTION

On August 5, 1952, I operated upon a 48-year-old patient for psychomotor epilepsy and removed his left temporal lobe. The patient, the second born of healthy parents, had had an uneventful infancy and was a model, perfectionistic student, usually at the head of his class. However, his father's untimely and accidental death forced him to leave school after the eighth grade and to enter the office of a local railway company. At about 18 he met a young woman whom he married four years later. Soon after his marriage he became restless and depressed; his illness was diagnosed as thyrotoxicosis, and a thyroidectomy was performed. At about the same time the first of his two sons was born; the second came two years later.

Following the operation the patient was able to work fairly well for six or seven years, but then became nervous and irritable and was demoted. He began to complain of paroxysmal pains about his heart. At these times it was noted that his head would turn to the right, a sweat would appear on his forehead, and occasionally he would mumble. As the attacks became more frequent he was studied at several hospitals, where diagnoses of idiopathic epilepsy and an infiltrating tumor of the right frontal region were entertained. He refused an operation or x-ray therapy. He was finally admitted to a state mental hospital because of aggressiveness and hostility, but after a few months was discharged and returned home.

The neighbors had always considered the patient to be a kind and considerate husband, quite in love with his wife, although during his attacks he became irritable and quarrelsome. His "spells," which occasionally occurred three or four times a day, were variously described—the most common, a sudden stiffening of the body, associated with groping movement of the hands. At these times his eyes would stare and seem to bulge, and he might make weird sounds; then suddenly he would regain his normal appearance and ask what had happend and what he was doing. Perhaps two or three times a year he stiffened, lost consciousness and fell. Once in a long while he might walk about muttering to himself, quite oblivious of his environment.

The neighbors, knowing that the patient sometimes became abusive, and having been asked by the patient's wife that, should they hear quarreling, they not interfere unless specifically requested to, were not unduly excited when, at midnight of December 27, 1946, they heard screams in the house and the sound of someone falling. They did, however, call the police.

The police officer was admitted to the house by the patient, whose clothing was covered with blood. He pointed to an adjacent room, where his partly clothed wife lay, both eyes blackened, two long knife wounds in the back, and 30 stab wounds in the chest. The patient had no emotional reaction to this scene of carnage, but sat staring glassily out of the window. From time to time he muttered that he had been framed, and asked that his brother and his lawyer be telephoned. Although his clothing was saturated with blood, a medical examination disclosed no wounds. He was taken to the local police station, where he suddenly began to stare into the distance and to

[1] Aided by a grant from the National Institutes of Health. Presented at the Annual Meeting of the North Pacific Society of Neurology and Psychiatry, held at Harrison Hot Springs, British Columbia, Canada, April, 1961.

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tear and pull at his trousers for a few minutes.

The next day his brother showed him newspaper accounts of the killing, but the patient could not believe that his wife was dead. A few weeks after the murder he was asked if his wife had been to see him and he replied, "No, that worries me. I would like to know how she is." When committed to a mental hospital he repeatedly told the physicians: "The other patients tell me that my wife is dead, that I killed her, but I couldn't have. She's the dearest thing that God ever gave a man. I just couldn't have killed her." When asked about the events of Christmas, 1946, the patient stated that he remembered Christmas and the night following, but could recall nothing else until his confinement in the police cell.

The opinion of the court psychiatrist and the staff of the mental hospital was that the patient had killed his wife in a postictal automatism. He was therefore committed. From that time until the operation in 1952 the patient had many attacks similar to those described, during some of which he became quite hostile and aggressive. In electroencephalograms a left temporal spike was noted, as were slow waves in the right frontal region. Because of the nature of the attacks, and the left temporal lobe spike, a left temporal lobectomy was advised. For a short time following this procedure he seemed definitely improved—his attacks were less frequent, and he was less aggressive. In the years between 1952 and 1960 he deteriorated slightly and became somewhat paranoid. In the summer of 1960 he apparently fell and fractured his hip, and had a very slow recovery from this injury. He died on October 2, 1960. At autopsy the cause of death was said to be septicemia due to ascending pyelonephritis. On examination of the brain, the left temporal lobe was ablated and over the right temporal lobe there was a superficial angioma (Figure 1).

Perhaps the right temporal angioma was related to the epilepsy, but in life, except for slow waves in one record, no clinical or electroencephalographic evidence of its role in the epilepsy was noted. Had the patient accepted the medical recommendation, made two years before his homicidal attack, to have the right frontal region explored surgically, the lesion might have been found and removed and—who knows?—perhaps the attacks might have ceased. This brief history illustrates the modern version of a case of epileptic furor.

PSYCHOMOTOR EPILEPSY

Although only in the last decade or so have these attacks of furor been recognized for what they are—temporal lobe discharges—they have for centuries been considered to be epileptic manifestations. And the individual committing criminal violence during them has been exonerated of responsibility. In fact, Delasiauve (2), in a book on epilepsy printed in 1854, devoted a chapter to the legal responsibility of the epileptic, and cited a number of instances in which an individual, during or after an attack, had committed a crime of violence. Among them was the case of a young epileptic butcher who went into a forest and shot his uncle under the delusion that the latter was planning to kill him. Evidence that the individual was actually in a convulsive episode at the time of the shooting was far from established. Again, a chronic epileptic, in the presence of a woman, assassinated and mutilated an 11-year-old girl. It was subsequently shown that the accused was fond of children, and was well acquainted with and friendly toward the child's family. During the two days before his crime he had had a number of severe convulsive attacks. In the analysis of this case, the frequency of the convulsions, the stuporous state of the accused thereafter, the absence of any motive for the crime (which was carried out in broad daylight), and the ferocity of the act indicated that the murderer was bereft of his senses.

In 1872 Kellogg (5) reported the case of

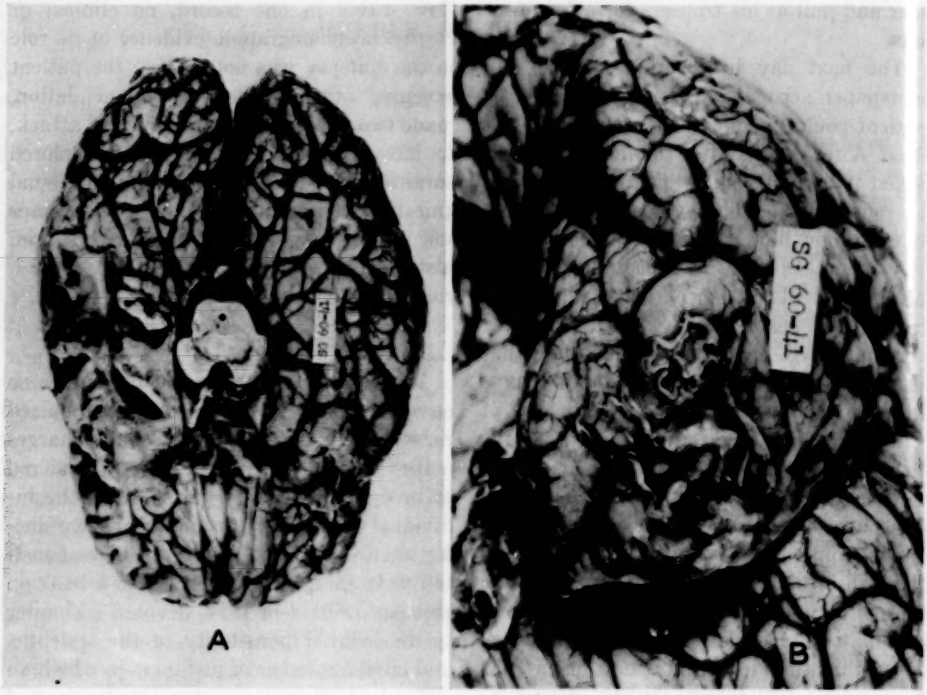

FIG. 1. A: Basal view of brain, showing left temporal lobe ablation. B: Enlarged view of right temporal lobe, demonstrating the superficial angioma. (Courtesy of Dr. R. Lindenberg.)

a 40-year-old man who had had convulsions at monthly intervals for ten years and who, at four o'clock one morning, arose from his bed and fell to the floor. His mother and two sisters put him back into bed. Shortly afterward he seized a fire shovel, rushed upon his sisters and beat them over the head. In the melee the stove was overturned and the house caught fire. The man then slashed the throat of one of his sisters with a razor. He was noisy and violent for ten to 12 days, and quieted down only after a series of convulsions. After he had fully recovered he was very distressed when, upon reading an account of his assault on his sisters, he became aware of what he had done.

Lennox (6) stated that he knew only two patients who exhibited evidence of criminal violence associated with epilepsy. He reported the case of a 22-year-old veteran who, while visiting an aunt, beat her to death, took her money, placed her body in the baggage compartment of a car and drove from a suburb of Boston into Maine, where he again bludgeoned the body and drove on until he was arrested. This man had a chronic epileptic father and had had at least two convulsive attacks, one at the age of two and one at 11 years. The patient disclaimed any memory of the events surrounding the murder, and stated that on one occasion he had found himself in a different state without knowing how he got there. An electroencephalogram showed an abnormal build-up on hyperventilation but no other abnormality; an intravenous injection of Metrazol caused an increase in voltage but no seizure discharges. A jury found the patient guilty of murder; he was sentenced to death, but the sentence was commuted to life imprisonment.

The frequency of such criminal acts i-

however, relatively low. Alström (1) found that, in a series of 345 male adult epileptics, only 5.1 per cent in the idiopathic group had penal records, whereas 4.8 per cent of all people in Sweden had such records. For patients with symptomatic epilepsy with mental derangement and neurologic defects, the incidence of a penal record was approximately 10 per cent. The condition must have existed, since in 1874 Maudsley (7) stated: "Whenever we meet with isolated acts of violence, outrages in persons, homicide, suicide, arson, which nothing seems to have instigated, and when, upon attentive examination and thorough inquiry, we find a loss of memory after the perpetration of the act with a periodicity in the recurrence of the same act, and a brief duration, we may diagnose larval epilepsy."

The condition is also well known in legal circles; more and more frequently, defense attorneys seek to establish a diagnosis of epilepsy for their clients.

On a rainy Saturday in June a few years ago, a 52-year-old man and his wife, who was perhaps a year his senior, drove from a cocktail party to their farm, some ten miles outside a large Eastern city. After a somewhat heated discussion the husband suggested that they go back. He preceded his wife out of the farmhouse and started down the steps to the driveway. As he did so, he was struck in the right temple by his wife's handbag, a soft one with a metal fastener. That was the last thing he remembered until he heard the caretaker of the farm, who lived in an adjacent building, calling to ask if everything was well. According to the husband's story, he then picked up his wife's battered, bloody body, which had been lying against a blood-spattered tree, carried it to his car and drove away. At about daybreak he put the body on the side of a road under construction and covered it with her coat. Following this he drove to his apartment, changed his clothes, went to his office and wrote some letters. At about noon he telephoned his brother in an adjacent city to tell him what had happened and to say that he was going to commit suicide. He called the police and drove back to where he had left his wife's body. He sat in the car with a .32 revolver in his hand until the police arrived and persuaded him to reconsider. He was then arrested and charged with murder.

The defense lawyer contended that the patient was subject to epileptic attacks. In confirmation, testimony was presented that the man had a history of having been knocked out on several occasions, and that in playing football he had suffered a broken jaw and had been unconscious for perhaps eight hours. It was stated that after one scrimmage he was found twitching spasmodically, with his eyeballs rolled back. It was also attested that at times he would drop to his knees as in an attitude of prayer. It was further testified that one month before the tragedy, while at a cocktail party, he had stopped in the middle of a sentence and stared ahead. On another occasion he seemed to have a fit of coughing; he became red in the face and his eyes bulged, whereupon he left the room and returned quite subdued after five minutes or so. It was further testified that his father, 79 years of age, had had several episodes of loss of contact with his environment.

Several electroencephalograms, made under varying conditions, revealed no abnormalities. Medical testimony was obtained from a number of specialists in the eastern part of the country, one well known neurologist testifying that the episodes previously mentioned were epileptic, but that he could not be sure the patient had been in an epileptic attack at the time of the brutal murder. The case, which was heard before three judges, was finally decided without more than passing consideration of the epilepsy issue.

The background of this patient was obtained from a number of sources. He had been a relatively good student who, somewhat early in life, had developed tuberculosis for which he had had prolonged treatment. During his entire life, however, he had been known for his hot temper, which

had got him into many fights. His family physician, who had attended both him and his wife for many years, and who was never called to testify at the time of the trial, emphatically stated that at no time had he had any clinical evidence of epilepsy.

The case illustrates the difficulties of disproving the diagnosis of epilepsy in a court of law. Had the above-mentioned patient had a few nonspecific changes in his electroencephalogram, such as occur in 15 per cent of the general population, it might have been impossible to convince the court that the accused did not suffer from epilepsy.

"14 AND 6" PSYCHOPATHY

Recently a somewhat specific electroencephalographic pattern has been found in a high percentage of young individuals with severe behavioral disturbances. Although this pattern appears in about three per cent of the general population, it occurs in 60 to 70 per cent of teen-agers confined to institutions for behavioral disturbances (10). This pattern, called "14 and 6 per second positive spikes," can usually be brought out only in sleep records (Figure 2). It is found in young individuals, and is present in the posterior parietal and temporal regions.

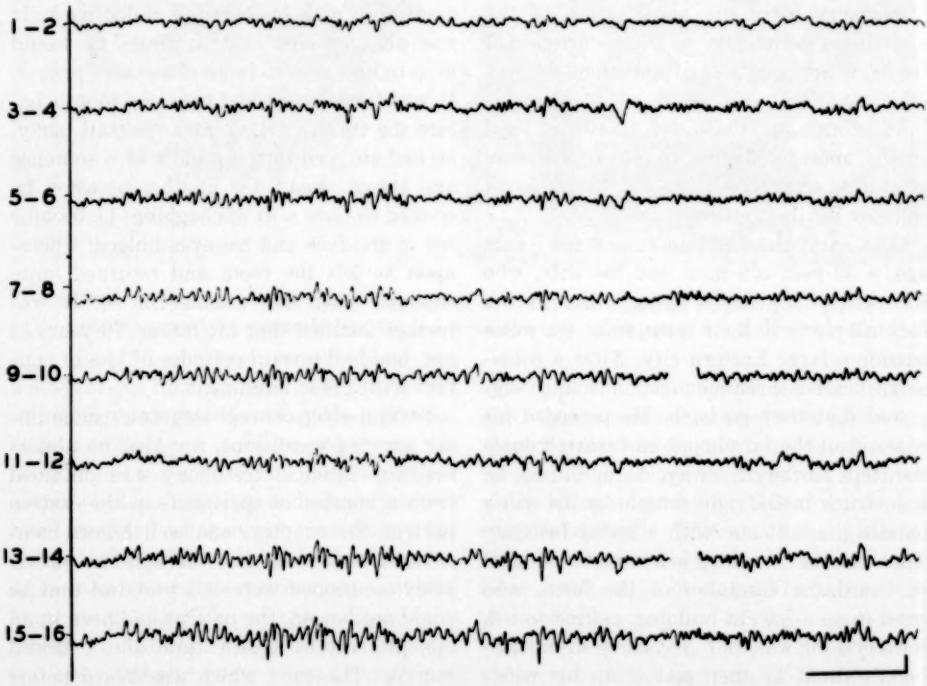

FIG. 2. Parasagittal montage, showing the 14 and 6 pattern of positive spikes. The horizontal line at the right base represents an interval of 1 second, the vertical one a calibration of 50 μv.

1–2 Bifrontal leads.
3–4 Biprecentral leads.
5–6 Bicentral leads.
7–8 Biparietal leads.
9–10 Bi-occipital leads.
11–12 Bitemporal leads.
13–14 Bisupra-auricular leads.
15–16 Bisupramastoid leads.

Gibbs and Gibbs (3), who originally described the pattern, considered it to be evidence of a hypothalamic or thalamic type of epilepsy. However, most electroencephalographers (4, 8) find that it correlates with one of three types of disorder: an overt epilepsy, a behavioral disorder, or such autonomic dysfunction as headache, abdominal pain or dizziness. It is thus related to some underlying pathologic condition, which may be characterized by one of several clinical manifestations. Gibbs and Gibbs stated that 15 per cent of patients with 14 and 6 per second positive spikes have attacks of rage. They reported that four patients had committed murder during an attack. One, a 47-year-old woman married to a brutal husband, was knocked down by him one day and told he was going to kill her. That night she shot him five times and killed him, later claiming that she had no memory of her act. The following day she recognized her surroundings, but was confused for several days. She had a typical 14 and 6 positive pattern. Schwade (9) reported a matricide by a 13-year-old boy who was able to describe the shooting. He had a 14 and 6 per second positive spiking but no evidence of epilepsy.

The writer once examined a teen-age boy who, while committed to an institution for delinquents, encountered a smaller boy he had never seen before and drowned him in a nearby stream. He told his story without any sign of remorse or emotion. He had a 14 and 6 pattern in his electroencephalogram.

Such behavioral disturbances are not confined to homicide. A 24-year-old married woman, held for cashing a forged check, told of having had so many traffic accidents that in the course of a year or so she had paid fines amounting to 900 dollars. On one occasion she became enraged at a physician who was treating her and threw a scalpel at him. Another time she made a gesture of suicide by jumping from a car (but without injury). She once worked in a circus for a period of three weeks, then one morning wakened unable to remember any of the events since she had joined the troupe. She recalled clearly the incident for which she had been apprehended. Although she denied any knowledge of having made out and signed the forged check, she described going to the store where she usually cashed her husband's pay checks, receiving the money in denominations that she was able to report exactly. She remembered taking it home and hiding it. She insisted that she had meant to give it to her husband, but was apprehended before he came home. She had a 14 and 6 pattern in her electroencephalogram.

DISCUSSION

Society has recognized, in its statutes against criminal or dangerous acts by its members, that there are individuals who are subject to periods of loss of awareness during which they may not be responsible for their behavior. Thus epileptics, who in the furor of their seizures or in the subsequent confused period have violated the laws of the land, have been exonerated of their crimes on the basis of irresponsibility. The concept is somewhat similar to the principle established in the famous McNulty case—that a person of diseased mind, unable to distinguish right from wrong, should not be held to be legally responsible for his acts. In the case of the epileptic, the principle assumes that an individual carrying out purposeful and well executed physical acts may yet be unaware of their significance and unable to control their execution. In one of the cases cited by Lennox and Lennox (6) the judge instructed the jury: "If it appears from the evidence that the mind of the accused at the time of commission of the act charged was in a diseased and unsound state, and that the disease existed to a degree that for the time being it overwhelmed the reason and judgment

and obliterated the will to such an extent as to cause her to act without being conscious of the act itself, again the act was not that of a voluntary agent but was the involuntary act of the body without the concurrence of the mind directing it, and the patient named cannot be held responsible."

In guiding the due processes of law, the neurologist and psychiatrist may render valuable service if they can establish certain criteria which the legal profession can use to assess an epileptic factor. Let us postulate the criteria to be considered in attributing a crime of violence to an epileptic state:

1) That the patient was subject to *bona fide* epileptic attacks. In the typical case, this assumption might be proved by an accurate play-by-play description of at least two attacks. In atypical cases, the nature of the spell may be determined only by a physician's observation of an attack. At times, the fit may have to be artificially induced by Metrazol or other drug, so that its course can be studied. If this is done, care must be taken to assure that the activating agent is given in minimal doses, and that a nonspecific effect has not been obtained.

2) That the spontaneous attacks of the individual are similar to the one which allegedly occurred at the time of the crime. In other words, if the individual is alleged to have had a psychomotor attack at the moment of the crime, he should in the past have had such psychomotor spells, for epileptic manifestations tend to have the same pattern in different attacks in the same individual.

3) That the period of loss of awareness alleged to have been present is commensurate with the type of epileptic attack the individual had. Patients having focal motor seizures with a Jacksonian march do not have appreciable impairment of consciousness during the local jerkings, and may have no loss of awareness until the entire body is involved in the seizure. Many patients with petit mal epilepsy—simple staring or absence—do not lose contact in their attack. On the other hand, psychomotor attacks are notorious for the confusion which follows even a brief lapse, and which may be the only noticeable evidence of the fit. Thus, focal motor attacks may pass through their entire sequence without any loss of consciousness, whereas temporal lobe seizures may have a degree of unawareness disproportionate to their motor phenomena. If the individual has had a series of epileptic attacks, a relatively long period of unconsciousness or confusion may follow.

4) That the degree of assumed unconsciousness is commensurate with the degree of unconsciousness the patient has had in previous attacks. If in his attacks the individual usually is confused and out of contact for some hours, it would seem logical that a similar period might have elapsed at the time of his act of violence. It must be remembered that a patient in a psychomotor seizure appears to the bystander to be conscious, and that he does react to personal insults as if he appreciated them as such—so that he may seem to be motivated and yet not be conscious of the act.

5) That the EEG findings are compatible with the type of clinical disorder assumed to be present. Thus, if the individual is considered to have a temporal lobe epilepsy, a spike should be demonstrated in the leads from that area. Repeated normal EEGs should be construed as decreasing to a one-to-20 chance the possibility of epilepsy. A pattern of 14 and 6 per secend positive spike does not necessarily indicate an epileptic diathesis, nor is it conclusive evidence of organic brain damage. The diagnosis in such cases should be based upon the total picture, and not biased by the EEG picture.

6) That the circumstances of the crime are compatible with the assumption of a lack of awareness of the individual at the time of the crime. This implies that obvious motives were not present, that the crime ap-

peared to be senseless, that the mutilation was unnecessarily violent and extensive, that the murderer did not attempt to escape, that he acted as if the act was foreign to him, and that he had given no evidence of premeditation.

It may not be possible to try the crime on all of these criteria, but if several do not appear to be met, one should look with suspicion upon the epileptic basis of the homicide.

SUMMARY

Illustrative cases of psychomotor epilepsy are presented in which homicide was committed during the period of unawareness. The characteristics of such crimes are discussed by reference to the sparse literature on the subject.

The psychopathic behavior of individuals having an EEG pattern of 14 and 6 per second positive spikes is described, and examples of homicides are given.

Criteria are presented which should be considered in deciding whether a homicide is murder, or a result of an epileptic furor.

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OBSERVATIONS ON THE DURATION OF PHOTICALLY ELICITED AROUSAL RESPONSES IN DEPRESSIVE PSYCHOSES[1]

W. P. WILSON, M.D.[2] AND N. J. WILSON, M.D.

The search for objective evidence of a neurophysiologic change in manic-depressive disorders and allied states has been a relatively fruitless one to date. Although several authors have studied these disorders electroencephalographically (1, 9–11), no consistent changes have been observed. Special techniques of study have been used but have produced equivocal results (2–4). Hypotheses regarding disturbances in diencephalic function in depression have been formulated but, for the most part, have been based on the presence of clinical symptoms related to autonomic instability, and have not been substantiated by evidence of change within the central nervous system. This paucity of information is primarily a result of the lack of investigative techniques in clinical neurophysiology, and only when new techniques are developed is further investigation of mental disease possible. The use of photically elicited EEG arousal responses, which are in part a measure of the level of activity in one anatomically delineated system (3, 12–14), afforded an opportunity to study the central nervous system function in mental disease; accordingly, this study of arousal response duration in depressive psychoses was carried out.

METHOD AND PATIENT MATERIAL

The 16 patients utilized in this study were selected from a larger group (52 patients). All 52 patients were severely depressed males and females who had classic affective disorders characterized by a mood statement of depression, and such biologic concomitants of the disease as disturbances of sleep, appetite and sexual function. Patients were excluded if they had symptoms indicating disorders of content compatible with schizophrenia, or a long history of neurotic symptomatology before the development of the depressive illness.

All of the patients were physically healthy, and their food intake was considered to be adequate. Supplemental multivitamin therapy was instituted on admission to the hospital if the patient gave a history of marked weight loss. None of the patients had received a tranquilizer for at least ten days, nor had any received a barbiturate for 48 hours preceding the recording.

Electroencephalographic studies were carried out on all of the patients in the larger group. Twelve chlorided silver electrodes were applied in standard head positions as determined by measurement, using the 10–20 system. Frontal, central, parietal, occipital, middle temporal, central vertex and ear electrodes were used. A control recording, using three runs, was obtained on a Grass III D electroencephalograph. Arousal responses were then elicited by our previously described technique (13). The intervals between flashes were randomly varied from 5–15 sec., with a mean interval of 10 sec.

Each record was analyzed visually and the arousal response measured to the nearest 0.1 sec. The values thus obtained were recorded and a mean value was calculated for each 30-sec. interval during the five minutes. Mean values for each 30-sec. interval for the entire group were calculated. Values for the control subjects were those obtained in Group 1 of our previously reported study.

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These subjects were healthy college students and hospital employees with a mean age of 27 years.

RESULTS

From the outset it was apparent that the number of usable records to be obtained in the study would be limited. In spite of careful explanation of the procedure and the establishment of good rapport with the patients, nearly all were quite fearful and/or tense at the time the recording was made. The high level of muscle tension seen in half the patients was often intensified by the photic stimuli. Restlessness and concomitant movement artifact also tended to interfere. A low alpha index was also frequently observed. As these factors were uncontrollable, it was necessary to discard all but 16 of the records. This low percentage of usable records is in contrast to our previous studies, where 60 to 70 per cent of the subjects screened had satisfactory control records. The mean age of the 16 patients was 49 years.

The data are summarized in Table 1.

The arousal response duration of the 16 depressed patients had a mean value for the entire five-minute period of 1.18 sec., which was considerably longer (0.39 sec.) than the control value of .79. These were significantly different at the .001 level of confidence.

Inspection of the individual records of the patients revealed a marked intensification of the periodic variation of the response. In many subjects, initial responses with a duration of 5–8 sec. were seen, soon followed by responses as short as 0.4 sec. and, in one instance, several zero responses. This widening of the range of responses was such a striking finding that, in two patients, further investigative effort was devoted to determining the effects of mental activity on the patients' alpha activity. These patients were therefore asked to perform difficult calculations and to ruminate about emotionally significant conflicts which were part of their preoccupations. In neither instance was there any change in alpha activity.

TABLE 1

Mean EEG Arousal Duration for 15 Control and 15 Depressed Subjects

Minutes of Recording	Control	Depressed
0 –0.5	1.18	1.62
0.5–1	0.92	1.57
1 –1.5	0.70	1.07
1.5–2	0.74	1.02
2 –2.5	0.80	1.20
2.5–3	0.74	1.26
3 –3.5	0.80	1.04
3.5–4	0.75	0.93
4 –4.5	0.67	1.06
4.5–5	0.55	1.02
	0.79, M	1.18, M

DISCUSSION

An earlier study demonstrated a decrease in the duration of the arousal response following the administration of a phenothiazine (14). The change may be assumed to represent a decrease in the level of reticular activity. Morrell (10) observed an increase in the arousal response duration following the administration of an amphetamine, which could be related to an increase in reticular activity. This increase is similar to that seen in the responses of depressed patients. However, in contrast to Morrell's observations, where periodic variations were decreased by an amphetamine, the periodic variations seen in the depressed patients were markedly intensified.

In an earlier report we pointed out that alterations in the duration of the arousal response cannot be ascribed solely to influences on subcortical neurons. It is therefore unwise to ascribe the changes seen here to changes in the level of activity in the reticular system of these depressed patients. Nevertheless, these studies suggest that involvement of the reticular system is possible, and, further, that the decreased en-

vironmental awareness and altered sensory perceptions of depressed patients may be related to increased inhibitory influences of the ascending reticular system on specific afferent transmission. This hypothesis is tenable in the light of the findings of previous investigators (5–7) that stimulation of the reticular system simultaneous with the stimulation of a peripheral nerve decreases the amount of information reaching the cortex, and that depression of the system with barbiturates increases the amount of information reaching the cortex (8). It is our hope that further studies now under way will shed some light on the question of the origin of the increased duration of the arousal response and its possible relationship to specific afferent transmission.

Finally, these changes apparently are not related to diet or to previously administered drugs, since these factors were well controlled. The alternate possibility—that the difference in ages of the depressed patients and the control subjects was responsible for the increased arousal response duration—may be discounted, since in another, as yet incomplete study we have found no increase in the duration of the response with age.

SUMMARY

Electroencephalographic arousal responses were elicited with single photic stimuli in 16 patients with psychotic depressive illnesses. The mean arousal response duration was significantly increased when compared with a control group of normal subjects. The changes observed are considered to be related to changes in the level of responsiveness of the neuronal groups involved in the elaboration of the arousal response.

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A BUFOTENIN-LIKE SUBSTANCE IN THE URINE OF SCHIZOPHRENICS

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In 1955 Bumpus and Page (1) concentrated acidified human urine at a low temperature under reduced pressure and extracted the residue several times with acetone. The purified acetone extracts were fractionated on an aluminum hydroxide column and the fractions tested biologically on Venus heart. The biologic activity showed three peaks: the corresponding fractions gave spots on the paper chromatographic assay which corresponded to those produced by serotonin, N-methylserotonin and bufotenin, respectively.

In 1956 Rodnight (10) carried out similar studies, combining absorption on cationic ion exchange resins and elution with paper chromatography. In about one-quarter of the urines the chromatogram showed a very slight indole spot in the bufotenin region. When bufotenin was added to the urine extracts, the same spots enlarged. Nevertheless, Rodnight concluded that bufotenin could not be detected in the urines. He also suspected that the spots observed and tentatively identified as bufotenin by Bumpus and Page might have been caused by tryptamine that, in alkaline solvents, gives spots with an Rf quite similar to that of bufotenin.

Fischer *et al.* (1959, unpublished) studied chromatographically the indolamines of human urine. These studies formed part of a research program of the graduate course in psychopharmacology at the Faculty of Medical Sciences of the University of Buenos Aires in 1959. Batches of five and 10 L. of pooled normal human urine were saturated with sodium carbonate and extracted by butanol. The butanol extracts were concentrated under reduced pressure and the residues extracted by acetone. The acetone extracts were centrifuged, the clear liquid was concentrated again and the residue extracted with acetone. The same process was repeated three times and the final residue was dissolved in one ml. of acetone, centrifuged and assayed by ascending (monodimensional and bidimensional) and circular paper chromatography, using butanol-ammonia and propanol-acetic acid as solvents and dimethylaminobenzaldehyde as a color-developing agent. Several spots of different colors were observed; one of them corresponded to 5-hydroxytryptamine, another (a very slight one) to bufotenin (color, Rf). The addition of 5-hydroxytryptamine and bufotenin produced no new spots, but the corresponding old ones were enlarged.

Fischer (5) thought that schizophrenic symptoms might be caused by an increased production of bufotenin. In 1959 Erspamer (3) considered the possibility that qualitative aberrations in the biosynthesis of serotonin, such as formation of N-methylated derivatives, might play a role in psychiatric disorders. To test the hypothesis, we assayed a number of urines of schizophrenic and other subjects with the method outlined above, but using only one L. of urine in each case. The final volume of the acetone extract was one ml. (corresponding to a total concentration of 1:1000), and 0.1 ml. was employed for each chromatographic assay. In five cases of schizophrenia we invariably obtained a characteristic spot corresponding to that of authentic bufotenin as to color and position, whereas in four Ss without psychiatric disturbances no such spots were present (6, 7).

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The procedure employed in this study was very similar to that used in 1960 by Gessner *et al.* (8) in their work on the metabolic fate of bufotenin administered to mice. When their paper was recently brought to our attention we felt that their method might have some advantages over ours, so we adopted parts of it (alkalinization to pH 9 by sodium hydroxide instead of saturation by sodium carbonate, continuous extraction with butanol instead of repeated shakings).

METHOD

One liter of urine was alkalinized to pH 9 with a diluted aqueous sodium hydroxide solution and extracted with butanol (in the first assays, by shaking twice with 150 ml. each, later continuously during three hours). The butanol extract was then concentrated under reduced pressure at 40–50° and the residue extracted several times by acetone, with occasional heating. The acetone extract was centrifuged and concentrated to dryness, and the extraction, centrifugation and concentration were repeated two more times. The final acetone extract was then concentrated to one ml. A total of 0.1 ml. was put on a paper strip (Whatman No. 5; 1.5 cm. wide and 30 cm. long) with a capillary pipet in small portions and tested by ascending chromatography with butanol-ammonia (butanol saturated by concentrated ammonia solution) as running solvent (24 hours). We later extended the running time to 36 hours, thus achieving a better separation of the spots. Color was developed by spraying with a two per cent solution of dimethylaminobenzaldehyde in 1.2 n hydrochloric acid and occasionally also with diazotized p-nitroaniline. The spots were observed immediately, then after five minutes of heating to 110°C., and finally after 24 hours.

RESULTS

We examined 15 urines from hallucinating or catatonic (one) schizophrenic patients and found in 14 a spot corresponding to that of authentic bufotenin (Rf: 0.84; color, violet-mauve), appearing only after heating of a few minutes (usually five). When bufotenin was added, the spot was enhanced. In certain cases we observed a band occupying the upper region of the chromatogram instead of the ordinary, rather round spot; in other cases the spot was very slight.

Added tryptamine formed a spot of different Rf (0.74) and color (purple-violet, changing to green after 24 hours), appearing immediately after spraying with dimethylaminobenzaldehyde. When diazotized p-nitro-aniline (a reagent for hydroxyindoles) was used, bufotenin gave a red spot. This also occurred with the urine extracts of schizophrenics, whereas tryptamine gave only a very pale yellow coloration.

In samples of the urine of ten *Ss* who were not schizophrenics, no spot corresponding to bufotenin was observed. Indeed, the whole upper part of the chromatogram was usually empty.

In every urine, dimethylaminobenzaldehyde revealed the yellow spot of urea (Rf: 0.27) and an unidentified red spot (Rf: 0.34). In ten schizophrenics and five *Ss* without mental disturbances, the blue spot of serotonin (Rf: 0.49) was visible.

DISCUSSION

Bufotenin, or N-dimethyl-serotonin, is present in the secretion of toads (12), in the fungus *Amanita* (13), in the seeds *Piptadenia peregrina* (11), and in the nematocysts of sea anemones (9). In human subjects it causes psychotomimetic phenomena (4). Also, after the administration of bufotenin monkeys behave in a way very similar to that observed after the administration of N-diethyl-lysergamide (2).

It is evident that the chromatographic assay alone, as used in our studies, cannot prove with absolute certainty the chemical nature of the substance in question. It is therefore necessary to amplify our proceedings before affirming definitely that bufo-

tonin is a characteristic ingredient of the urine of schizophrenics. We are at present engaged in such experiments, in an effort to establish the identity of this substance by using a number of revealing agents and other procedures besides the chromatography, and also to isolate the substance from a great amount of pooled urine.

We feel it is suggestive that a spot indistinguishable by two different reagents from that caused by authentic bufotenin was present in the urine of 14 of 15 schizophrenics, but not present in ten urines of non-psychotic persons. We believe our results support the possibility that an altered indole metabolism with an increased production of N-methyl-serotonin or N-dimethyl-serotonin may cause schizophrenic symptoms.

Since this manuscript was written some new results have been obtained. The number of schizophrenics examined was increased to 26, and positive results were obtained in 25. These cases varied widely as to age and length of hos-

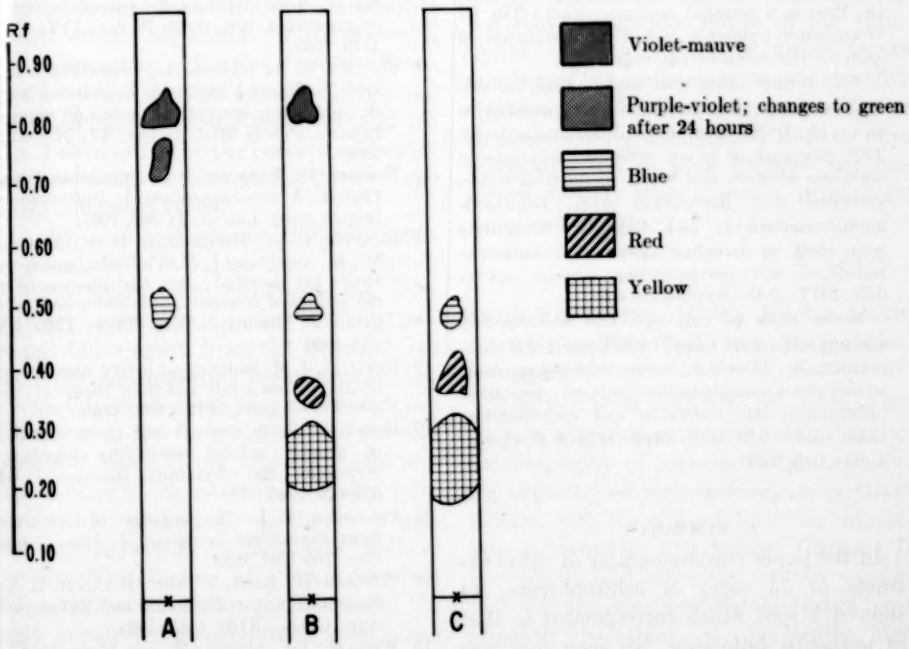

FIG. 1. A: Chromatogram of a mixture of authentic bufotenin, tryptamine and 5-hydroxytryptamine. B: Chromatogram of a hallucinating schizophrenic subject. C: Chromatogram of a non-psychotic subject.

Chromatogram A:

Rf 0.84: Violet-mauve spot of authentic bufotenin that appeared after five minutes of heating.

Rf 0.74: Purple-violet spot of tryptamine that turned green after 24 hours.

Rf 0.49: Blue spot of 5-hydroxytryptamine.

Chromatogram B (a hallucinating schizophrenic subject):

Rf 0.84: Violet-mauve spot that appeared after ten minutes of slight warming.

Rf 0.27: Yellow spot of urea.

Rf 0.34: Unidentified red spot.

Rf 0.49: Blue spot of 5-hydroxytryptamine.

Chromatogram C (a non-psychotic subject):

The same spots except for that of bufotenin.

pitalization, but all were men presenting hallucinations or catatonia, and none had received any treatment for 15 days before the test. The control cases (a total of 15), consisting of alcoholics, epileptics, neurotics and some clinically normal people, interned or living in the same hospital, consistently gave negative results.

The urine extracts of three cases (two of catatonic schizophrenia, one of the paranoid form) were also tested with the acetic acid-isopropanol solvent (12 vol. of isopropanol, 3 vol. of glacial acetic acid and five parts of distilled water), and the spots were revealed by dimethylaminobenzaldehyde, diazotized nitraniline and 2-6-dichloro-p-benzoquinone-4-chlorimine. (The last reagent was recommended to us by Dr. Reio in a personal communication.) The Rf of authentic bufotenin and of the corresponding spots of the urine extract was 0.85.

Five further cases were studied with the solvent mixture called Dipream (recommended to us by Dr. Irvine in a personal communication). This consisted of 1 vol. concentrated aqueous ammonia solution and 9 vol. of distilled water, saturated with diisopropyl ether. Dimethylaminobenzaldehyde and diazotized nitraniline were used as revealing agents. Rf: authentic bufotenin and corresponding urinary spots: 0.53; 5HT: 0.45 (tryptamine 0.58).

Seven cases of catatonic and hallucinating schizophrenia were tested by bidimensional chromatography (first run, acetic acid-isopropanol; second run, ammonia-butanol solvent; diazotized nitraniline). Rf: bufotenin and corresponding urine spots: 0.85, 0.89; 5HT: 0.75, 0.40 (tryptamine, 0.92, 0.69).

SUMMARY

In the paper chromatograms of urine extracts of 15 cases of schizophrenia, 14 showed a spot which corresponded to that of authentic bufotenin. No such spot was found in the chromatograms of ten cases who were not schizophrenics.

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STOKES, ADRIAN. *Greek Culture and the Ego.* Tavistock Publications, London, 1958. 101 pp. 15s. (G. B.)

Subtitled *A Psychoanalytic Study of an Aspect of Greek Civilization and Its Art*, this short book is a continuation of the studies of Mr. Stokes applying the teachings of Melanie Klein to the psychology of art. Classical Greek art is here the field of observation, taken as a whole, with some specific illustrations. Mr. Stokes's thesis is stated at the beginning: "I consider this accommodation [of the Good and the Beautiful] to issue from an adjustment between the good objects of the super-ego and the ego. I shall say that the concept of beauty projects, not the ego-ideal, but the ideal ego as an integrated system."

A Kleinian commentary follows, interdigitated with examples from the Greek cultural achievement. Although the commentary encourages a rather unwieldy language, quite apart from the technical vocabulary, an interesting argument emerges, the usefulness of which may not require full adherence to the theory. Art—and pre-eminently classical Greek art—originates in a reprojection to the external world of a fully concretized, integrated "ego-figure," a unified organization of the introjects. Following Mrs. Klein further, Mr. Stokes describes the development of the ego in terms of the early introjected objects—some good, some bad—with the ensuing "paranoid" and "depressive" attitudes of the ego in respect to the introjects. The potential chaos so represented (and manifested in psychosis) is limited only to the extent to which fusion with the good objects predominates in the ego, and the tendency toward splitting of the inner world into its original warring components is thereby restrained. So the Greek figure enhances our experience through its re-presentation of the "whole" ego-figure: "Shape, pattern, growth, rhythm, interlocking parts of whatever kind, restore ourselves to ourselves."

Despite the inclusion in the book of a few essays on linguistic and scientific development, no light seems to be shed on the causes of the special properties of Greek art, with its relative subordination of the superego. Indeed, this phenomenon, while apparent in the best representations of archaic and classical sculpture, with its sublimely humanistic dignity and serenity, tends to obscure our notice of the "Dionysiac" element in Hellenic culture, to say nothing of its disintegrative and horrible elements, which demanded quite different treatment. *Why* it happened thus requires more attention to wide cultural and historical matters than a purely psychologic study can afford.

S. A. Leavy

SMITH, SIR SYDNEY. *Mostly Murder.* David McKay, New York, 1959. 318 pp., illus. $4.95

Here is a book written to sell, to hold the reader's attention and to entertain him, an autobiography of perhaps the greatest living authority on forensic medicine in Great Britain, with the added fillip of an enthusiastic foreword by Erle Stanley Gardner. It is a popularized, smoothly written account that will firmly support the interest of most armchair detectives. A consistently high level of blood-letting obtains.

The author, born in New Zealand and educated at the University of Edinburgh, began his professional career during World War I as the principal medico-legal expert of the Egyptian Ministry of Justice. A decade later he returned to Scotland and held the chair of forensic medicine at Edinburgh from 1928 until retirement in 1955. During a span of more than 40 years Sir Sidney's expertise in ballistics and in the manifestations of arsenic poisoning brought

him into dozens of well known murder cases as principal medical consultant. They are all vividly presented here, together with histories of homicide by strangling, beating, stabbing and fire—a veritable saturnalia of gore. Sir Sydney's especial skill seems always to have been that of identifying the victim of a murderous assault, and the cause of death, from the least of evidence—a fragment of bone, a hank of hair. These are truly absorbing accounts.

This book will very likely have a wide audience. It is, needless to say, not written as a detached scientific communication, though parts of it might well have been. Neither the text nor the illustrative photographs, however, will appeal to those readers with bland palates.

H. A. R.

BECKER, ERNEST. *Zen: A Rational Critique.* Norton, New York, 1961. 192 pp. $4.00

In the years following the Second World War, American servicemen of a philosophic bent returned from occupation duty in Japan with tales of a strange and wonderful institution apparently quite unlike any heretofore known in this country. This institution of Zen Buddhism appeared to be not only a unique form of personal salvation, but also a world view of revolutionary impact—and, it almost seemed, whatever the particular exponent wanted it to be when he was talking about it. Since a vast amount of nonsense has been written in the last decade proclaiming the virtues of Zen, a reaction against this adulation was only to be expected. A blow in this cause was struck by Arthur Koestler's recent *The Lotus and The Robot*; a second is struck by the author of this book, an anthropologist with a pronounced interest in psychoanalysis.

Dr. Becker states at once that his purpose is "to show that Zen really is a denial of life, a negation of the Western ethic of individuation and autonomy," and that "Zen is basically a technique by which to achieve a mental breakdown of people so that they can be made to accept a new ideology." Unfortunately, the tools Dr. Becker brings to his task seem no more suitable than those of the uncritical idolators who have preceded him. Like them, he has based his argument on a handful of secondary sources; like them, he has accepted as "Zen Buddhism" whatever someone else said it was; like them, his scholarship has been less than careful. For example, he attributes recital of the Nembutsu, the religious formula of the Shin sect, to one of the Zen schools, an equation roughly comparable to making recital of the Hail Mary an integral part of Quaker meditation.

An even more serious failing for his undertaking, however, is Dr. Becker's apparent lack of personal contact with anyone who has completed Zen training, or even, perhaps, with anyone who has had any first-hand acquaintance with it. Since a better understanding of Zen seems most likely to be achieved by empirical study, and an anthropologist would seem to be particularly well suited to collecting such data (which are far more readily available now than even ten years ago), Dr. Becker's failure to present personal observations by someone who has had any experience with Zen is a matter for regret. There is evidence that Zen Buddhist training represents a distinctive form of influence, although one of considerably less potency than Dr. Becker has been led to believe. My own impression—one shared by Fromm and Horney, among others, on the basis of contact with people who have undergone Zen training—is that these people show distinctive personality features which they attribute to their training. These are far from systematic impressions; quite possibly they are in error. But far more urgently needed than another armchair essay on what Zen is about is an empirical study of its effects. Lifton's work on Communist "Thought Reform," quoted with approval

by Dr. Becker, is a model of such work. Starting with intensive individual study of people who had undergone "Thought Reform," Lifton proceeded to relate it to Chinese history and culture in an original synthesis which represents a significant advance in our understanding of techniques of influence. Such a study of Zen would seem more than justified in the current state of our knowledge—and ignorance—of this much misunderstood institution, and could make a real contribution to our understanding of human behavior in general, and to psychotherapy in particular. By contrast, Dr. Becker has done no more than rearrange ideas which he has imperfectly comprehended.

Albert Stunkard

BENNETT, EDWARD. *Personality Assessment and Diagnosis: A Clinical and Experimental Technique.* Ronald Press, New York, 1961. viii + 287 pp., illus. $8.00

Despite its rather imposing main title, this book is actually a presentation of a single specific assessment procedure. The procedure could be classed as "objective" (since it has standardized response categories and is subject to quantification), and "direct" (since it asks of the person directly what one wishes to know). The author speaks of it as a method for "mapping subjective feelings." In a forced-choice situation, the testee is required to make certain statements about his attitudes toward himself and others. What prevents distortions in self-report? Dr. Bennett seeks to prevent distortions by giving the testee a forced-choice among unpleasant alternatives—for example, selecting negative traits that describe oneself, on the assumption that one has no easy way out. This is to the good as far as it goes—but it goes only so far. The alternatives are not equally unpleasant, and the method ends by giving some unknown admixture of "subjective feelings" (or beliefs or attitudes), and what the testee is willing to report. This, then, is a self-report technique with the benefits of simplicity that such techniques have, but with incomplete control over distortion, a lack that has led others to rely on "indirect" techniques (including the projective tests, among others).

And that, it seems to this reviewer, is it. For the rest, the book is a collection of material sometimes loosely and sometimes closely related to the basic technique. It includes a totally unnecessary introductory section on personality theory, a section presented with a theoretical oversimplification and a pedagogic definiteness that make it completely inappropriate for the professional audience that would read the book for the assessment technique proper. Also, even for an introductory textbook discussion of personality, this one is both too definite and too superficial in dealing with the vast field it seeks to cover.

Some clinical sections are included, purportedly to show the applied uses of the technique, but the opportunity for careful validation of what the technique measures, by comparing it systematically to other aspects of the test records or case histories, is missed. Instead, case and test materials are listed, and the self-report is tacked on. The author succumbs to the easy lure of looking over the self-report and selecting (by what principles?) whatever he wishes. This material does not, as the author implies, give a "map" of feelings, but only some raw material for such a map. The mapping must be done by the examiner in his search for relationships among the various items of self-report. Despite the claim of objectivity and quantifiability, principles for such mapping are rarely given. We end up with only a list, one in which, to make matters worse, causal relations are found by some unknown means. (A person says he is insecure and also unmechanical; therefore, he is insecure partly *because* he is unmechanical in an industrial society.)

The two concluding sections on research

fare little better. Part of this material has previously been published. Some (adequate) test-retest reliability data are given, but validation is still lacking. What about the person being reported in this research? The way he feels? Thinks he should feel? Is willing to report? Such questions are not even approached.

Why this was published as a book is unclear: the theory is not at a level for the research audience, the research has in part been published elsewhere, and the clinical chapters, while not of very high quality, shed no light even on critical aspects of the assessment technique.

What have we left? To this reviewer, it seems we have only an interesting assessment procedure, of some usefulness, but one that should have been presented in a brief article. The procedure is simply one among many currently available, but it has the advantage of being readily adapted to the study of a wide range of content areas—attitudes, self-concepts, moods, stereotypes—some of which are suggested in this volume.

Fred Pine

BRAZIER, MARY A. B. *The Electrical Activity of the Nervous System.* 2nd ed. Macmillan, New York, 1960. xiv + 273 pp., illus. $8.50

This second edition of Dr. Brazier's volume is still a small book, although a significant expansion of the 1953 version. The first third of the book, devoted to basic principles as derived from studies of nerve fiber, is virtually unchanged from the earlier edition. This is not a criticism, since the current hypotheses of excitation and transmission have not undergone any basic changes in the intervening years. However, some mention might have been made of the voltage clamp techniques of Hodgkin and his colleagues, and of the elaborations of the ionic hypothesis to explain the action-potential, refractory period and after-potentials.

The section on synaptic transmission has been enlarged, and now embodies information obtained from micro-electrode techniques applied to single cells. There is tacit acceptance of the concept of chemical transmission at synapses, with graded post-synaptic responses, both excitatory and inhibitory, providing the plasticity and the matrix for the complex subtleties of neuronal interactions.

The other major areas of expansion are found in sections on the reticular system and in the demonstration of centrifugal controlling influences exerted by the CNS on peripheral receptors.

The volume will continue to be of interest to all students of the nervous system, but is designed primarily for medical students and graduate students in physiology.

J. K. Merlis

GORLIN, MORRIS, ED. *Maimonides "On Sexual Intercourse"* (Medical Historical Studies of Medieval Jewish Medical Works, Vol. 1, Fi 'L-Jima, translated from the Arabic). Rambash, Brooklyn, New York, 1961. 128 pp. $10.00

This slim volume is actually more the work of the translator and editor, Morris Gorlin, than of Maimonides, since only about 15 pages of its text are devoted to the latter's original work, whereas the rest of the book contains (a) spurious work attributed to Maimonides, (b) excerpts from some of his authentic works, (c) source material from various other authors, and (d) connecting commentary by Gorlin.

Taken in its own right, *Maimonides "On Sexual Intercourse"* is largely a treatise on aphrodisiacs and anaphrodisiacs which, unfortunately, faithfully follows most of the superstitions of its (twelfth century) day. Typical among its contentions are these:

"The sperm is actually the surplus of the foods, which remains from what the organs require at the time of the third digestion; therefore the body is emaciated when it squanders thereof, in addition to the other

damage deriving from squandering" (p. 25).

"The physicians have already made mention of the things which especially weaken sexual intercourse. Sexual intercourse with a virgin, an old woman, a young girl who has not yet attained sexual maturity, or a woman who has already for several years abstained" (p. 31).

"Mutton, young doves and all brains, particularly the brains of hens, doves, and sparrows, and rooster testes are very beneficial for the increase of the sperm and its production in all temperaments and ages, and so the marrow of bones, the yolk of hen eggs, dove eggs, pullet eggs, sparrow eggs and milk as it issues forth from the udders, and of plants, the turnip, the onion, and particularly their white variation, the fennel, the mint, the chickpea, the kidney bean, the bean, sesame and asparagus and of the tree fruit; seed of dry almond, of hazelnut, of grape, and of pistachio and the kernels of the great pines and Indian nut..." (pp. 33–34).

Mixed in with these kinds of unscientific dicta are a few sage observations, including (as Gorlin enthusiastically points out) the psychosomatic interpretation that intercourse is not merely a physiologic process over which the person has no control, but also a psychologic process which depends upon his emotional responsiveness. These few wise notations in no way compensate for the fact that Maimonides' work is, of course, largely nonsense, and that it has little more than interesting historical value today. Gorlin has done an excellent job of putting the Maimonides treatise in historical perspective and in publishing much of the interesting source material connected with it. His commentary, though awkwardly written, is valuable. The Maimonides work itself may be of value for scholars; otherwise, the book can be read quickly as a bit of curiosa that holds virtually no clinical or scientific interest.

Albert Ellis

GURIN, GERALD, VEROFF, JOSEPH AND FELD, SHEILA. *Americans View Their Mental Health: A Nationwide Interview Survey.* Basic Books, New York, 1960. xxxv + 444 pp. $7.50

This monograph reports a study sponsored by the Joint Commission on Mental Illness and Health, set up by the Mental Health Study Act of 1955.

A questionnaire designed by the University of Michigan Survey Research Center was used in interviewing 2,460 Americans over 21 years of age living at home. The goal was the measurement of self-estimated personality adjustment and methods used to handle emotional problems. The subjects were selected so as to represent the total population as to age, sex, education, income, occupation and place of residence. The interviews lasted one to four hours, and were conducted by people specially trained in such interviewing.

The questions had been pre-tested on a small group, and were so worded as to elicit "true" attitudes and feelings and yet obtain data—an enormous amount of data—that could be coded and analyzed statistically.

The data are clearly presented; the style is logical and lucid, although in some sections the book is ponderous and over-written. The authors present a discussion of the interview questions, the method of coding results, and the statistical data in table form, with an interpretation of the significance of the results. They explore adjustment in terms of how Americans feel about happiness and anxiety, the relationship of marriage, children and the family, and the role of introspection in the self-image. The findings are then examined in terms of age, sex, income, education and occupation.

The section dealing with symptoms was based on a factor analysis of a symptom checklist which had been used in large part in the Midtown and/or the Sterling County studies. The analysis yielded four major factors:

1) Psychologic anxiety (related to insomnia, nervousness and loss of appetite).

2) Physical health (related to pains and ailments, and interference of ill health with work).

3) Immobilization (related to reported difficulty in getting up in the morning, and to feelings of not being able to "get going").

4) Physical anxiety (related to shortness of breath and other extreme physical symptoms.

These factors were analyzed against the aforementioned dependent variables.

The second part of the book considers the various ways in which people go about resolving their mental health problems. (Definition of terms, *e.g.*, "nervous breakdown," was left to the individual respondent.) Some highlights of the findings:

One out of four subjects felt he had had problems for which professional help was needed, but only one out of seven had sought such help. Most had consulted clergymen (42 per cent) and non-psychiatric physicians (29 per cent). Some who wanted help (14 per cent) did not seek it because of a reported sense of shame. The data suggested that availability of psychiatric treatment did not in itself spur people to seek such help. The role of prayer, and active versus passive methods in handling everyday problems, were also studied.

Those of our colleagues who must grapple with state legislatures for funds to support psychiatric facilities will find this book helpful, for it contains the force of public opinion systematically collected. The scope of mental health needs from the constituent's point of view is clearly presented.

The findings will also be useful to administrators who must plan the training and teaching of professional personnel. Increased emphasis on training in pastoral counseling for clergymen is needed, as is the education of non-psychiatric physicians in psychotherapeutic techniques, since these people constitute the first line of defense in dealing with emotional problems.

The authors obviously have a broad and sophisticated view of behavior; they have asked questions which allow for meaningful conclusions, scientifically arrived at and with sound attention to the rigor needed in such work.

The book is an interesting collection of Americana, a comment on some aspects of the American's view of himself, in mental health terms, at midcentury. We can hope that another such study will be made in the future, to observe the modalities of change.

Myron Eichler

ENGLISH, O. SPURGEON, HAMPE, WARREN W., BACON, CATHERINE L. AND SETTLAGE, CALVIN F. *Direct Analysis and Schizophrenia.* Grune and Stratton, New York, 1961. vii + 128 pp. $4.25

The saga of John Rosen could be done justice only by a wide screen, living color and epic Hollywood production with a firm but sensitive director. For this reason it is extremely difficult for scientists to be scientific around Rosen: he is effervescent, kaleidoscopic, and *never not interacting.* Many psychoanalysts have simply written him off, and a few have openly damned his work. However, the vision and even the bravery of Spurgeon English have been responsible for a documentation of Rosen's work that should be the source of important study for years to come.

Rosen's therapy is difficult to evaluate, not only because of his unique and sometimes disquieting personality but also because Rosen represents an era. He caricatures the current trend in the psychotherapy of schizophrenics, which is away from the "poor gentle violet" approach; this means that opinions on his therapy are difficult to tease out from the observer's bias about treatment favored today. Of the four contributors to this volume, English does the best job in separating Rosen the man from a psychotherapeutic approach which embodies principles that can be effective even

if one does not copy Rosen. For example, the *reductio ad absurdum* technique, and the constant switching of learning contexts, are techniques which can be described in general communication or transactional terms and which will, I am sure, help us all in being a little more resourceful.

This book is something of a disappointment if one has already read Scheflen's masterful report on his research on Rosen's work (*A Psychotherapy of Schizophrenia*, published in 1960 by Charles C Thomas). However, there is an interesting clinical-anecdotal flavor to the four reports, and an opportunity to see how four different psychoanalytically oriented individuals approach the same data. English seems clearly the most interested and the most forgiving. Settlage is more concerned about psychoanalytic theory, and in certain ways his contribution reveals the difficulties encountered in applying psychoanalytic conceptualizations to the complex spectacle of Rosen treating a schizophrenic. For example, do Rosen's id interpretations aid the patient in repressing, or do the interpretations decathect tightly bound complexes? Rosen (who, as several of the authors point out, is no theorist) has felt that his interpretations release the need for certain defenses; but it can also be argued that the shocking effect of Rosen's statements may cause the patient to strengthen his defenses. It seems to me that neither of these viewpoints can be considered adequately without a full understanding of the context in which the interpretations are uttered, and this context is a complex one indeed. It may even be misleading to call Rosen's interpretative statements to the patient "interpretations." Rather, they should be viewed as a series of verbal and non-verbal messages offered in the framework of an interpretation. In view of the help our colleagues in the social sciences have been trying to give psychoanalysts, it would seem unjust to leave out the many "non-analytic" factors which abound in the situation of Rosen's treatment. Among these would be the power available in the treatment setting, the fact that the patient is always in the role of a guinea pig with a multiplicity of observers, and that many or most seem to be lower class or lower middle class patients receiving free treatment from an eminent authority. For example, I have seen films in which Rosen makes an interpretation (or confrontation) to the patient and then turns to the audience and explains what he is doing. Does this behavior on his part reinforce or undermine the effect of "the interpretation"? It seems to me that it forces upon the patient the need for an adjustment between these two positions, and thus the need to resort to another level of communication. The patient can afford neither to prove Rosen wrong nor to accept completely his psychotic position in the goldfish bowl. When Settlage (page 70) states, "Crucial to my understanding is the mechanism of introjection," I can only shrug my shoulders. It helps me little that he differentiates among incorporation, introjection and identification. My lack of enthusiasm is not for these well defined psychoanalytic terms, but rather in the direction: introjecting what? When Rosen presents so many aspects of himself at the same time, and when the situation in which the patient finds himself is so complicated, I do not know what he "introjects," if anything. Similar arguments can be raised about the use of terms like "good," "bad," "love," mentioned in page after page of this book. Bacon (page 61) describes how Rosen gave a patient the formula $E = MC^2$ and thus protected her from voices so "she was free to form a relationship to Doctor Rosen and loved him as her rescuer." I do not know whether one "loves" a magician, or whether one settles on a *modus vivendi* that includes not hearing voices when the magician forbids them.

I doubt if psychiatrists will ever pay their debt to John Rosen. For one thing, if they tried to, he probably would not accept

it. It is, however, encouraging that under the leadership of English the work of this remarkable man is being scrutinized for the benefit of us all.

Don D. Jackson

ALT, HERSCHEL. *Residential Treatment for the Disturbed Child.* International Univ. Press, New York, 1960. xii + 437 pp. $7.50

The Jewish Board of Guardians of New York have traditionally been articulate in describing their efforts to meet the mental health needs of children. Alt's book is clearly a fine addition to and a cumulative summary of what has been previously published. Although the focus is on the residential treatment center, Hawthorne Cedar Knolls, a purview is provided of the kinds of services needed by any community to deal with the seriously disturbed child. Portions of the book are devoted to planning, financing, physical plant, integration of treatment approaches, use of small residential units, hospital wards, outpatient services, foster homes and so on.

The meat of the volume, however, is the matter-of-fact description of the evolution of Hawthorne Cedar Knolls School from a training school to a treatment center. Hawthorne, established in 1906, has always been in the forefront of current rehabilitative philosophy. The author provides a detailed discussion of the various problems, solutions, and changes in program from 1935 to date in the attempt to create a total therapeutic situation for the rehabilitation of delinquent children. The historical perspective of 25 years of seeking "the total therapeutic situation" provides valuable insights for those coming more recently on the scene.

Over the years a number of studies designed to identify the characteristics of Hawthorne children have led to the conclusion that the social delinquent is gradually being replaced by the more severely disturbed, aggressive, defiant youngster. There has also been an increase in the number of fragile, non-delinquent but deeply disturbed neurotic and psychotic children.

The basic theoretical frame of reference for Hawthorne embodies a complete synthesis of psychotherapy with the total living and educational experience of the child. Each child's situation is studied comprehensively, with classic psychoanalysis as the guiding orientation. Such a treatment philosophy becomes a reality when incorporated into the attitudes of the adults and into every phase of the child's management. Perhaps one of the most valuable sections of the book is the description of the interaction over the years between the theoretical treatment concepts, staff training and the eventual influence on the actual experience of the children. The essential problem is the extent to which clinical, educational and child-care functions can and should be maintained as distinct and autonomous activities while at the same time serving as integral components of a treatment effort.

Such a problem eventually resolves into the training of the workers. Hawthorne has found that even the most highly trained of professional workers seldom have the requisite specialized knowledge and skills for a residential setting, and require in-service training. But the child-care function is the foundation for total treatment, and discussion of the responsibilities, prerogatives, status, training and supervision of the cottage parents is exceptionally valuable.

It is sobering to the beginner in residential treatment planning to learn that residential treatment is a pluralistic problem, and that the environment must be differentiated and individualized for the various diagnostic groups. The description of the struggles of Hawthorne to provide for a number of distinct treatment units within the one larger institution is alone worth the price of the book. Four widely varying groups of children have been involved: the adolescent delinquent boy, the psychotic or

neurotic child, the young schizophrenic child and the delinquent girl. The program for each of these is described quite profitably.

Three detailed case studies are provided to illustrate the treatment methods. The "how to do" books in this field are relatively few. *Residential Treatment for the Disturbed Child* is perhaps less dramatic and the theoretical insights are less exciting than is the case with some of its predecessors. Because of the publication policy of the parent agency, many of the principles of planning and therapy seem like old and familiar friends. But child psychiatry and those engaged in planning of community programs owe a vote of thanks to the author for providing this practical, common-sense guide to residential treatment.

Frank T. Rafferty

MULLAN, SEAN. *Essentials of Neurosurgery for Students and Practitioners.* Springer, New York, 1961. xi + 273 pp., illus. $6.75

Since this book was written for the student and practitioner so that they might fit each new clinical experience into its modern framework, it may be pertinent to inquire as to the present sources of their knowledge of the medical specialties—in particular, neurosurgery. A few decades ago, small pocket synopses of nervous diseases distributed by the pharmaceutical houses served as texts for many students. Today, the majority of students rely upon systems of general medicine and surgery for their information. Few can name a textbook of neurosurgery or neurology. However, this is not too unsatisfactory a way of reading a specialty, since most neurologic sections in textbooks of medicine and surgery are authoritatively written.

One might ask, then, what advantages a book on neurosurgery has over the sectional discussion of the subject in a system of surgery. Obviously the book allows a more detailed account of the subject—perhaps an attempt to integrate neuro-anatomy, neurophysiology and neuropathology into the clinical material; perhaps an effort to introduce points of differential diagnosis into the discussion; perhaps the presentation of more illustrative material, or perhaps the incorporation of more technical operative details into the text. Unless the extra space available is used for one or more of these purposes, the monograph has no particular advantage, from the standpoint of the student or general practitioner, over the sectional coverage of the specialty.

The author of such a treatise has particular responsibilities arising from the fact that his is not a sophisticated audience. It is his duty to present an unbiased account of his subject, for the reader lacks the background for critical assessment of technical material. Accurate data which can be incorporated without reservation into the thinking of the student or general practitioner are essential. Moreover, the subject matter must be predigested, so that it consists of simple, proved and authoritative statements; hence, such a textbook must of necessity lag about five years behind the wavering frontiers of knowledge.

The author of this textbook apparently believes that the student and general practitioner prefer a discussion of the specialty of neurologic surgery in broad generalities rather than in particularities. This is apparent in his discussion of the surgical afflictions of the nervous system, in the schematic line drawings which he uses to illustrate the anatomy of the nervous system and the pathologic alterations due to disease, and in the fact that only a few references (usually to monographs) are given for additional reading after each chapter. Within this framework the field of neurologic surgery, including a brief look at its history, is adequately covered in the 14 chapters.

The neurologic surgeon will find much that is of interest in this book, but not much

that is new, and will find a number of points with which he will disagree. The statement that hearing loss is the earliest symptom in acoustic neurinomas is contrary to the classic teachings of Cushing, and out of line with the experiences of most neurosurgeons. If the author is correct in saying that "it is quite absurd to test the strength of a patient's ankle by the examiner's hand" (p. 175), neurologists and neurosurgeons are playing the fool every day. Even a neurosurgeon would find it difficult to understand the outlet syndrome of the thorax from the brief description given in a half-dozen lines. Striking omissions are noted: radioactive scanning, a common procedure in many hospitals at present, is not mentioned, nor does the author say anything regarding the use of a stimulator in the surgery of peripheral nerves. Many names are misspelled in the text, and typographic errors are rather frequent.

On the whole, this book presents the essentials of neurologic surgery, and can be recommended to anyone wanting a general description of the subject.

A. Earl Walker

ESCALONA, SIBYLLE AND HEIDER, GRACE MOORE. *Prediction and Outcome: A Study in Child Development.* Basic Books, New York, 1959. xvi + 318 pp. $6.50

Those readers familiar with the excellence of the authors' previous research reports and the quality of research conducted at the Menninger Foundation will expect to find exciting developmental ideas proposed and tested in this volume. Exciting developmental hypotheses are proposed in abundance; unfortunately, few appear to have been adequately tested in this research.

The authors have attempted to forecast various behavioral personality characteristics of preschool age children on the basis of observations made when these children were less than eight months of age. It would be difficult to locate an area of research of greater scientific and practical significance than that of identifying infant behaviors which allow prediction of dimensions of subsequent development. Although prognosis is crucial to social scientists and clinical practitioners alike, the data and processes upon which predictions are based are seldom explicitly and exactly specified. It is to the great credit of the authors that they have attempted to grapple with perhaps the most meaningful and yet the most difficult problem encountered in understanding human development.

The researchers are of demonstrated competence and the research issues of crucial importance. Yet the research product fails to meet its promise. While methodologic rigor is difficult to maintain in a study of complex predictions of human behavior, nonetheless a series of methodologic inadequacies diminished the potential contribution of the study. These shortcomings may be classified as follows:

1) Definitions of the variables are seldom offered.

2) The relationship between theoretical concepts and observable phenomena is often either unspecified or equivocal.

3) The inter-rater reliability of independent judges' evaluations of criterion observation is inadequate.

4) Problems of the empirical validity of predictor and criterion variables are not raised.

5) Although an attempt is made to describe the general process of writing the predictions, the rationale for many of the predictions is not clearly defined.

6) The rules for judging predictions as correct or incorrect are not sufficiently amplified.

9) Granted that elaborate statistical procedures would have been inappropriate to these data, more extensive data analysis could have been performed.

Some of these methodologic problems arise from the fact that predictions are

tested by comparing observations derived from two separately planned and conducted studies. The possible connection between the two studies was not planned in advance, but resulted from the fortuitous use of the same sample of subjects. The lack of planned co-ordination between the predictor and criterion studies may have some advantage in eliminating certain contaminating biases. However, many earlier prediction studies have indicated the critical need for carefully selecting, defining and measuring criterion variables to provide the exact information required by the prediction. The disadvantages of performing prediction studies upon data derived from accidentally juxtaposed or sequential studies probably far outweigh the advantages.

Much evidence is accumulating to demonstrate the value of employing groups of assessors in personality research. However, after jointly conducting their study, Escalona and Heider found that they could not agree upon a common approach to analysis, interpretation and form of presentation, and so they have apparently reversed the technique of combining the skills of experienced psychologic evaluators by presenting separate sections within the book. Thus this research report presents two separate analyses of the data derived from two separate studies. Beyond common introductory remarks about their broad theoretical framework and the background of the study, no attempt is made to contrast the two approaches carefully and to assess their relative merits.

A likely reaction to these methodologic objections is that they are relevant only to "statistical prediction," and that this book describes "clinical," not statistical, prediction. Dr. Escalona states frankly that the "aggregate of predictive statements for each child is a heterogeneous assortment including simple theoretical derivations, even simpler empirical observations which were merely extended and applied to the child in question, and a peculiar variety of focused fantasy products which often reached some degree of complexity," and that "predictions took this shape not because I thought this good scientific procedure but because I could not do it in any other way..." (p. 22). The predictions were made through a "subjective experience of emergence" (p. 23). The yield from such an approach would seem to assess the clinical facility of the individual predictor, rather than the predictive validity of specified theoretical concepts.

It is unfortunate that these methodologic difficulties mask the important assets of this research. Intriguing substantive speculations and conclusions are offered concerning the variables of activity level, motor development, interests, affects, social behavior, speech and symptomatology. The book is a storehouse of ingenious hypotheses and insightful observations. It should definitely be read, not because it offers adequate tests of the hypotheses, but because it proposes exciting new directions for thought about child development.

Gerald S. Lesser

CHAPANIS, A. *Research Techniques in Human Engineering.* Johns Hopkins Press, Baltimore, 1959. xii + 316 pp. $6.00

The title of this fine book by a pioneer worker in the field of human engineering is inclined to be misleading. To this reviewer it suggests a handbook for the expert in engineering psychology which provides information on the latest in equipment and procedure for handling particular human engineering problems. It is no such thing. It is, rather, a clearly written primer of the experimental method, illustrated by examples from human engineering. It could have been illustrated as readily by studies from other areas of experimental psychology, physiology or experimental medicine. It thus makes valuable reading for the spe-

cialist—*e.g.*, the engineer, the physician—whose formal education has not included training in the logic of experimentation, but who finds himself in the role of researcher. It is well suited for use as a supplementary text for a beginning course in experimental psychology, and this reviewer plans so to use it.

Research Techniques in Human Engineering is a revision of a monograph, *The Design and Conduct of Human Engineering Studies,* prepared for the military services. There are eight chapters. Only three —one including materials on time study and motion study, one describing the critical incident technique in accident research, and the final chapter, on articulation testing—focus on human engineering in a narrow sense. Even here, no clear distinction is made between human engineering and the closely related area of human factors research. (This reviewer prefers to limit the term *human engineering* to the consideration of human factors in machine design and layout.) The chapters on experimental design and the psychophysical methods are excellent introductions to these topics. It is regrettable that no attention is given to trend analyses, on the one hand, or to scaling methods, on the other, since the methodologic repertoire of no experimenter—either basic or applied—is complete when he has learned only to measure psychophysical thresholds and to perform "yes-no" experiments that simply identify variables as affecting or not affecting the dependent variable under consideration. Perhaps the best chapter in the book is Chapter 6, which deals with the problems associated with using human subjects. Even the experienced behavior scientist would do well to review it periodically.

There is a lengthy discussion in Chapter 5 of the question of realism in experiments, in which it is pointed out that realism usually must be sacrificed to achieve adequate control. Meanwhile, it is obvious that an experiment, even though perfectly executed, is of dubious value unless its results may be generalized beyond the specific conditions of observation. The writer would have added clarity to the discussion if he had distinguished between a realistic and a representative experiment. The latter need not be done under "real life" conditions, but need only simulate in some way the critical features of the situation for which generalization is desired.

The book is well written. It contains, however, a faint flavor of condescension, an occasional tendency to write down to the reader, seen in spots as needless prolixity. For example, on pp. 77–78 a good half-page is devoted to saying that, in collecting data on accidents, it is important to employ personnel who are well trained and conscientious. And there is an occasional careless phrase. For example, on p. 17 it is stated that the pooled statements of several observers are uniformly more valid than those of a single person. Statements are either valid or not valid, and it is likely that the author was thinking of reliability when he commented on validity.

These comments notwithstanding, *Research Techniques in Human Engineering* is well done and warrants the attention of any person who wants to learn how to do competent research in the behavioral sciences.

William Bevan

FRIED, EDRITA. *The Ego in Love and Sexuality.* Grune and Stratton, New York, 1960. viii + 296 pp. $5.50

With an academic background in psychology, Dr. Fried (now Senior Supervisor and Lecturer at the Postgraduate Center for Psychotherapy, New York, and a member of the faculty of the Albert Einstein College of Medicine) has set herself the task of observing and describing how the ego operates in love and sexuality. Psychoanalytic concepts seem to form a vague theoretical substrate, and several chapters

deal with such dynamic problems as ego regression and the relationship of hostility to sexuality. Beyond a simple presentation of the libido theory and a vigorous attack on the culturally oriented psychoanalytic schools which are said to reject nonrationality, Dr. Fried does not really come to grips with many of the theoretical problems that her subject entails.

A relatively simple level of presentation is consistently maintained; nonetheless, it is not easy to determine who might profit most from this volume. The intended audience is nowhere specified. Now and again a most provocative idea appears, but the book does not seem to have been directed to the serious scientific student. It could hardly be recommended as core reading to psychoanalytic candidates. Psychiatric residents or clinical psychology students might find herein some points of value about adult sexuality, but at the risk of having them embedded in something less than a full-bodied psychoanalytic presentation. In certain phases of their training and experience, social workers and seminarians might enjoy and, conceivably, profit from reading this monograph. Some aspects of the simplified presentation suggest that Dr. Fried was writing for the educated layman; a recurrent point of view much akin to "mental hygiene" makes one wonder whether it might be usefully recommended to newlyweds, or perhaps to some patients. But its approach is detached and coldly clinical, and so raises doubts about its appropriateness as a source of sexual enlightenment for a lay audience.

The book is predominantly based on psychoanalytic psychotherapy, carried out in both individual and group settings. The operational meaning, the setting and the specific therapeutic techniques employed are, however, described only by indirection, and are not entirely clarified by the many brief case sketches. These latter illustrations imply a remarkable successfulness of Dr. Fried's therapeutic approach. Psychodynamic formulations, neat and tidy, abound in the highly abstracted case reports: they nicely illuminate the points made, but are shorn of the perplexing complexities that are most often found in clinical practice. Chapters deal with such aspects of sexuality as "unresponsiveness," "pains of boredom" and homosexuality. There is an almost exclusive preoccupation with adult and marital problems (one chapter is devoted to masturbation in adults); sexuality in childhood and in late adulthood is dealt with not at all, nor are such pathologic problems as fetishism and exhibitionism.

Clearly Dr. Fried means well for her readers, and in these pages has offered some of her rich clinical experience. She wants sex to be enjoyed; she favors the kind of personal fulfillment with which an arrested ego might interfere; she deals with some of the dilemmas sexuality poses in our lives and in psychotherapeutic work. Her book, however, is not in sharp focus, and clearly is not an exhaustive treatise on the ego in love and sex.

JELLINEK, E. M. *The Disease Concept of Alcoholism*. Hillhouse Press, New Haven, 1960. ix + 246 pp. $6.00

For a number of years Dr. Jellinek has played the role of elder statesman in the field of alcoholism. And let there be no mistake about it: unlike schizophrenia or manic depressive psychosis or anxiety neurosis or hysteria, alcoholism is somehow a field unto itself, even a way of life, absorbing the energy and a good deal of the emotion of many working within it. As a friend of the reviewer once put it: alcoholism is the only psychiatric syndrome which has literally developed a national movement.

Alcoholism as a clinical problem has a past and present history of inspiring loyalty or distaste; there are those who are earnestly devoted to the problem and those who scoff at its *aficionados*. To attempt to understand why alcoholism produces this

behavior in those who deal with it is a task for the social historian. Perhaps the solution is simple: as a pathologic behavior tied to issues of moral values and religion, to economics and law, to social custom and ritual, it has—deservedly—a unique place among the syndromes of psychic aberration.

Dr. Jellinek reviews the history of alcoholism-as-illness, pointing out that the "new approach" is really an old approach, renewed. He grapples briefly with the question of how to define *disease*, and ends with this: "A disease is what the medical profession recognizes." (What is there in the pragmatic American character that makes such definitions palatable? Intelligence is what the intelligence tests measure, and Dr. Conant has remarked that sometimes he is "ready to define education as what goes on in schools and colleges.") Since the medical profession has accepted alcoholism as an illness quite officially, therefore alcoholism *is* an illness. But alcoholism-as-illness or alcoholism-as-disease as opposed to what? There are at least three issues here, and Dr. Jellinek deals with the first and second briefly in a summary table (pp. 55–59).

1) There are those who take the position that alcoholism is a *symptom* and not a full-blown, self-contained clinical entity. Alcoholism, then, cannot be called a disease, any more than can a high fever or a headache or pain. Alcoholism, according to this view, occurs *because* the individual is neurotic or psychopathic or psychotic, and it is these diagnostic categories that constitute the disease condition, not the alcoholism.

2) Is alcoholism a disease as opposed to a sin? Not psychopathologic but immoral behavior? Few professional individuals hold this position, and even the churches have been quite active during the last decade in promulgating a view of alcoholism as psychopathology. As a result, perhaps, of a barrage of educational material (A.A. publications, television plays, articles in popular magazines), public views of alcoholism also seem to have shifted from sin to illness—although, as Dr. Jellinek wisely points out, this may very well be a kind of lip-service opinion. But public views *have* shifted as more and more people come to look upon schizophrenia, juvenile delinquency, even alcoholism, not as sin, or punishment for sin, but as deviant, abnormal behavior.

3) Finally, there is the question of whether alcoholism should be called a *disease* rather than *deviant behavior*. Here, the issues raised by Szasz in his paper, "The Myth of Mental Illness" (Amer. Psychol., **15:** 113–118, 1960), are relevant. He points out that "the norm from which deviation is measured whenever one speaks of a mental illness is a psychosocial and ethical one." What complicates the question is, of course, that alcoholism is not merely an acting out, not merely deviant drinking behavior, but also involves, as Dr. Jellinek lists them, "... tolerance, craving, habituation, sensitivity, compulsion, 'habit forming drug,' withdrawal symptoms, 'loss of control,' and so forth."

Dr. Jellinek states his own position quite clearly: "... Anomalous forms of the ingestion of narcotics and alcohol, such as drinking with loss of control and physical dependence, are caused by physiopathological processes and constitute diseases." It follows from his preference for a "physiopathological" explanation that he has little patience with or interest in a psychoanalytic view of alcoholism. The dogmatism of psychoanalysts about alcoholism (or any other issue) is not defensible, but at least two recent psychoanalytic papers have suggested some fresh thinking on the subject. Dr. Jellinek's conclusion—that psychoanalytic theories "... do not go beyond what the non-analytic psychiatric and the formal psychological theories have to offer" —gave me pause. As an interested student of these theories, I would ask him to be more explicit. I think that psychoanalytic theoretical writing about alcoholism has

virtually no overlap with "non-analytic psychiatric and the formal psychological theories."

Dr. Jellinek finds a learning-theory approach most promising, but the fact of the matter, as he points out himself, is that "the *initiating* process of addiction" is the concern of this theory, and it "does *not* exclude any other etiological theories" [italics added]. Psychoanalytic theory and learning theory are not necessarily competing attempts at description and explanation. If the process of learning how to be addicted were to be satisfactorily explained by learning theory, the question would still remain why individual X comes to acquire this learning and not individual Y—*i.e.*, what life experiences and group memberships have readied X to learn-how-to-be-addicted.

The author varies in his enthusiasm about different research approaches. Granted that a laboratory experiment with rats is not the equivalent of psychoanalytic interviews with patients, someone will have to convince me that a more rigorous laboratory experiment with 53 rats turns up more "significant" findings than a psychoanalytic study of 52 patients. I am not speaking against laboratory animal studies of complex behavior, but I am wary of the reasoning-by-paradigm which pervades animal work. The animal experiment defines terms like anxiety and fear and conflict more precisely, *i.e.*, operationally, but that is not to say that the rat is experiencing the human anxieties and fears and conflicts that initiate the process of addiction, according to the theory. Dr. Jellinek, like others who must evaluate "behavioral science," is more entranced with *science* than with *behavior*. Research is not necessarily "significant" just because it contains hard, statistically workable data. Psychoanalytic "research" papers do not follow the rules of scientific method, but, viewed as a potential source of hypotheses, they have their place. This point of view is particularly well supported in a recent, careful research study by Gibbins and Walters of the homosexuality issue in alcoholism, "Three Preliminary Studies of a Psychoanalytic Theory of Alcohol Addiction" (Quart. J. Stud. Alcohol, **21**: 618–641, 1960). These authors concluded that "while the experiments as a whole do not provide strong evidence for the psychoanalytic theory, the results suggest that this theory should not be lightly discarded."

All writing involves selection. Dr. Jellinek, holding the point of view he does, excludes some recent writing which would have been worth his attention,[1] but includes an uncritical reference to an anthropologic study[2] which has "clearly shown" a statistical relationship. However, as Dollard and Miller point out in *Personality and Psychotherapy* (McGraw-Hill, New York, 1950): "This study would have been more convincing if separate raters, unaware of the purpose of the study, had independently made the ratings of insobriety and of anxiety-provoking subsistence hazards." It is laudable to take a theoretical position and state it clearly and openly, as Dr. Jellinek has done, but let us recognize that a theoretical position tends to influence one's perceptions and interpretations.

The main strength of this book is in its global view of alcoholism. It is always humbling and a jolt to our ethnocentrism to be made aware of the fact that each of us knows well only one or two communities in one country, and that a whole lifetime of specialization may give us insight into a relatively small sample (smaller and smaller as the world population gets larger and larger). From his years with the World

[1] Two papers come to mind: Levy, R. I. The psychodynamic functions of alcohol. Quart. J. Stud. Alcohol, **19**: 649–659, 1958; and Witkin, H. A., Karp, S. A. and Goodenough, D. R. Dependence in alcoholics. Quart. J. Stud. Alcohol, **20**: 493–504, 1959.

[2] Horton, D. The functions of alcohol in primitive societies: A cross-cultural study. Quart. J. Stud. Alcohol, **4**: 199–320, 1943.